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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

ROBERT JAMESON,

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Mr Henwood's communication we are affraid will require to be illustrated by expensive plates. Mr Smith's interesting communication is somewhat in the same predicament. Other communications unsuitable for the Philosophical Journal will be returned to the authors.

ERRATA.

Page 46, foot-note, 1st line, *for* sine *read* eine, and same line, *for* gesamten *read* gesammten, 2d line, *for* Ertes *read* Erstes.
Page 358, *for in pari casu*, *read* in similar circumstances.

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Observations on Drift; on the Causes of Change in the Earth's Superficial Temperature; the Doctrine of Progression with respect to Animate Beings; Doctrine of Progression with respect to Inanimate Matter: being part of the Address delivered at the Anniversary Meeting of the Geological Society of London on the 26th February 1852. By WILLIAM HOPKINS, Esq., President of the Society.*

GENTLEMEN,—In the wide range which Geology now presents to us, it has not been without some perplexity that I have determined on the form of the Annual Address which I am now called upon to make to you. The more frequent precedent afforded by similar addresses would suggest a general analysis or review of what has been done, especially in our own Society, during the past year; and this appears to me one obvious and useful object of such addresses. At the same time I think it right that each of your Presidents in succession should judge for himself as to the manner in which he may best fulfil his mission, and adopt that course which he may feel himself capable of rendering most subservient to the progress of our science. You will recollect that during the past year we have been much occupied in discussing the superficial accumulations now generally designated as "drift." Our Quarterly Journal of the past year contains a considerable number of papers, and some elaborate ones, bearing more or less immediately upon it. It is a branch of our science, too, which has been making of late great progress, but in which much yet remains to be done before we arrive at a complete knowledge of the phenomena, and those sound theoretical views which may command something like unity of assent. For these reasons I have determined to make

* From a copy of the Address presented by the Author.

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this subject the leading one of my address. In doing so I shall not restrict myself to a mere analysis of the communications which have been made to us. I shall venture to criticise them with such freedom as may, I trust, require no further apology than that which the desire of advancing our science may afford. I shall also, before I enter on this more detailed analysis, endeavour to bring before you a general view of some of the more important parts of the subject, under the aspect which it now presents to us. Papers also on other subjects have been brought before us, which are far too important to be omitted in any general review of our proceedings, and to which I shall in the sequel direct your attention.

I. *Drift.*

If the period of the drift involved only a repetition of the action of those geological causes which we recognise in earlier geological periods, it would still have an especial interest, as approximating to our own times, and as less likely than those earlier periods to have the nature and character of its operations and phenomena masked by those of succeeding periods. But besides this, we have reason to regard it as a period of peculiar conditions, and of phenomena referable to peculiar causes, the study of which has opened to us entirely new views respecting the agencies which have so marvellously modified the face of our planet, by the continual transference of matter from one part of its surface to another. The study of this period has also led us to a knowledge of climatal conditions not before suspected, and to various researches into the causes which may have produced those conditions; and thus we have extended our knowledge of one of the most interesting branches of terrestrial physics.

There is perhaps no branch in which speculative geology has recently made more satisfactory progress than in theoretical views respecting the agencies by which the larger masses associated with the drift, the erratic blocks, have been transported from one locality to another. At the same time, no subject, perhaps, has been more characterised, in passing through its various phases, by extreme hypotheses and premature conclusions. When water alone was recognised as the means of transport, hypotheses were sometimes made respecting the magnitudes of single waves, and their passage even over elevated mountains, which nearly all of us should now agree in condemning as extravagant; and effects were attributed to them which, from the transitory character of any single wave, were not only improbable, but perhaps physically impossible. In the abandonment of such extreme hypotheses we have made a most salutary step. Nor was the introduction of the glacial theories of transport, by glaciers and floating ice, unattended by hypotheses, which might be deemed extreme hypotheses with

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as much propriety as those which were condemned as extravagant in the agency of water. It is manifest, however, that these extreme views are gradually but surely giving way in favour of those more moderate, and, as I believe, sounder views to which we appear to be rapidly converging.

The glacial theories of transport of erratic blocks made rapid progress among us soon after their first announcement, although received by many geologists in the first instance with great reservation. One reason of this reserve was, I imagine, the difficulty of conceiving a change of temperature such as required by those theories, exactly opposite to the changes which the geologist had ever contemplated—a change after the glacial epoch from a lower to a higher temperature. Increasing knowledge, however, of the causes affecting climatal conditions have enabled us to remove in great measure this source of doubt. Another reason for hesitation in accepting these theories was, perhaps, to be found in the incautious manner in which their claims were asserted by some of their first advocates, and the unlimited application which was made of them to account for the phenomena of transported materials of all kinds. Whatever truth might belong to the facts adduced in support of these theories, it was clear that much of the reasoning founded upon them was untenable. Overstrained applications, however, of physical theories, are almost the necessary consequences of their early reception by minds animated by an ardent zeal for the discovery of new scientific truths; and perhaps this tendency, in certain stages in the progress of science, may be almost necessary to counteract the hesitation of those whom natural timidity, or possibly severer mental discipline and more accurate physical knowledge, may have rendered too slow in the recognition of the germs of new theories, while supported, perhaps, by little of demonstrative evidence. All doubts, however, as to these theories being founded in truth, whether there might be more or less of exaggeration in the advocacy of them, soon gave way before the evidence collected by northern voyagers respecting the action of icebergs, and that supplied by Agassiz, Charpentier, Forbes, and others, who devoted themselves to the study of the constitution and motion of glaciers. Almost all geologists, I conceive, now agree in the opinion that both floating and terrestrial ice have played their part to a greater or less extent in the transport of erratic blocks.

The theories of Agassiz and Charpentier as to the causes of glacier motion have been refuted by the exact admeasurements made not only by Professor Forbes, but by those of Agassiz himself; and the speculative views of the latter philosopher on the former extension of glaciers over the surface of a large portion of the northern hemisphere are no longer received. But, gentlemen, geologists would be ungrateful if, while they acknowledge, as we

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all do, the great value of the researches of our countryman Professor Forbes on the Alpine glaciers, they should in any degree forget the debt they owe to the distinguished Swiss naturalist and his countryman, who were the first to point out the effects of glaciers in smoothing and striating rocks, to urge their effectiveness in the transport of blocks, and to indicate phenomena of a past epoch similar to those of the present time, in such a manner as to command the attention of geologists, and finally to lead to the adoption of our present views respecting the glacial epoch. It is especially to M. Agassiz, and his ardour in the pursuit of scientific truth, that we owe the first knowledge of this subject in our own country. His visits here, and the personal favour with which he was received among us, gave him frequent opportunities of expounding his views; and I cannot refrain on this occasion from expressing the delight with which I call to mind the open-hearted hospitalities which he exercised in the deep recesses of the Bernese Alps, and from testifying to the perfect unreserve with which he communicated his views to those alike who favoured or opposed them.

I have already remarked that water was formerly almost the only recognised agent in the transport of erratic blocks. On the introduction of the glacial theory it was superseded, and appeared to be almost forgotten; nor does it still seem to have regained what I conceive to be its just claims, in the minds of many geologists. On the abandonment, however, of some of the unreasonable claims of the glacial theories, and the distinct recognition of large portions of drift as subaqueous phenomena, the importance of currents as agents of transport gained more attention, though there are probably many persons who yet fail to realise in their own minds the enormous power which such currents may possess, even without greater velocities than may be easily allowed them. This power arises from the fact, which I have elsewhere demonstrated, that the moving force of a current, estimated by the weight of a block of any assigned form and material, increases as the *sixth power* of the velocity of the current. It is this which accounts for the circumstance that the same atmosphere which in one state of motion constitutes a summer breeze, but just sufficient to move the leaf or the flower, exerts at other times the almost irresistible force of the storm. It is on this account, too, that, reasoning from the power of ordinary currents of two or three miles an hour, we are liable to miscalculate so entirely the force of a rapid current.

I consider the distinct recognition of these three agencies of transport—glaciers, floating ice, and currents—as essential to the final establishment of sound theoretical views on this subject, and the great majority of geologists are probably prepared to recognise them to a greater or less extent. It is equally essential that we should be prepared to assign to each of these agencies its share in

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the great work of transport according to the characters of the transported materials; for it is alone by a careful study of these distinctive characters that we can hope to decide by what agent the transport has been effected. On this point there appears to be still much discrepancy of opinion, when the test has to be applied to individual cases. These differences of opinion seem to manifest themselves principally on questions relating to the action of water, either with reference to the form in which currents tend to deposit a general mass of drift, or to their effect in rounding and wearing the individual component parts of it, as compared with the tendency of other modes of transport to produce similar effects. It may be that we have not yet studied these effects as referable to different causes with sufficient care, or that we are still too much influenced individually by preconceived notions; but it is certain that different persons do draw very different inferences as to the mode of transport of a given mass of drift, from the characters which its component materials present. In some cases such inferences will probably ever remain doubtful, but in others there can be no reasonable grounds for doubt. Most geologists appear now to agree about what may be regarded as the two extreme cases, and admit small rounded pebbles as a proof of long-continued aqueous action, and very large erratics with perfectly unworn angles as equally indicative of transport by ice. If there be any among us not glacialists to this extent, I recommend them to the personal study of these blocks. I well recollect, in my own case, that after resisting all verbal arguments in favour of glacial theories, I stood at once convinced under the silent appeal of the *Pierre à bot* on my visit to that magnificent erratic of the Jura. In almost all the cases intermediate to these extremes, I fear we have much yet to reconcile before we come to any unity of opinion. And here, gentlemen, let us ask ourselves in the spirit of candour, whether one cause of this may not be found in our natural tendency to hold too pertinaciously to preconceived opinions. It will not be denied by any one, I imagine, that it would generally be the *necessary* consequence of a transitory current driving a mass of drift over a level surface, to spread it out in an approximately equable layer; while such a result could generally be regarded as only the *accidental* consequence of transport by floating ice. Such a layer would indicate the latter as a *possible* mode of deposition, the former as a highly *probable* one. When the glacialist contends for the *possible* rather than the *probable* mode, let him examine himself strictly whether he may not be unconsciously under the dominion of preconceived theoretical views. Again, the polishing of rocks and their striation in definite directions may be generally regarded as the *necessary* consequences of the passage over them of a large mass of ice, preserving its general direction of motion in defiance of merely local

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obstacles. Such effects *might* also be produced by the passage of masses of detritus. The former is a *probable*, the latter a *possible* mode of producing these phenomena. When the opponent of the glacialist, therefore, urges the latter against the former mode of action (except under some particular condition), let him also institute a self-examination as to whether he is exercising his unclouded and unprejudiced judgment. Gentlemen, I would exhort you earnestly to prosecute your researches and speculations with a fair and liberal feeling towards the views of others, and especially with an unflinching obedience to the laws of inductive philosophy. Every geologist, who takes an impartial review of the history of his own mind with reference to geological opinions, will probably feel that what is termed consistency of opinion would frequently have been in his own case persistency in error. I feel the more entitled to make these remarks, from the consciousness of having resigned much of my own early convictions respecting the glacial theory; and I make them in immediate connection with the subject before us, because I believe that much remains to be done in these superficial deposits before we can completely interpret them; and I believe also, that for our progress towards sound opinion and unity of view respecting them, ability and fidelity in the observer will scarcely be more necessary than that fairness and candour without which he will assuredly fail to bring his observations as true tests of the different views with which the subject is at present perplexed. Let us not seek for mere *possibilities* in support of antecedent opinions, but submit our views constantly to the test of enlarged experience and careful induction. There may be, doubtless, a stage in the progress of science in which new views, thrown out at random, and the advocacy of individual opinion with somewhat more than philosophical pertinacity, may be effective in the development of truth; but there is assuredly also another and more advanced stage of science, in which such habits of mind can only retard and embarrass its progress, and impede our arrival at those ultimate truths which it may be our object to establish. At this latter stage I believe the science of geology to have arrived; and if by these remarks I should induce one speculative geologist to watch with increased rigour the reasoning by which he arrives at his convictions, I shall perhaps have done more for our science than I can do by any detailed information which an occasion of this nature may enable me to bring before you.

I shall now direct your attention to some of the leading characters, of the great mass of drift which extends over so large a portion of northern Europe. And first I shall speak of the *striæ* which so abound in the northern part of the region in question. When regarded with reference to a limited area, their directions might be described as characterised by the law of parallelism; but

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when regarded with reference to the whole region, we find them really characterised by the law of divergency. To those observers who had not examined the striæ on the shores of the North Sea, some point lying to the north of those shores, and nearly in the direction of Spitzbergen, seemed best to represent the centre of this divergence; but subsequently M. Böhtlingk observed striæ descending from Kemi eastward to Onega Bay, on the shores of which it is situated; and on the northern coast of Lapland he also observed them descending from the high lands northward to the sea. These observations have also been corroborated by other observers. Around the district comprising the mountains of Scandinavia striæ appear to exist, directed to almost every point of the compass, and the characters of their divergency generally for the whole region may be considered as established.

The directions in which the detrital matter has moved in its transport across a particular locality cannot, of course, be ascertained with entirely the same accuracy as those of the striæ; but the erratic blocks can in numberless instances be identified with the rocks of a particular locality, and thus the *mean* direction in which a particular block has travelled, can be determined with great accuracy. All the blocks, however, originating in the same locality have not been transported in the same direction. M. Durocher has noticed especially a granular granite, easy to be recognised, of which the original site is in the department of Vibourg in Finland. The extreme directions in which the blocks have proceeded from this spot comprise an angle nearly equal to two right angles. The *mean* direction, however, of these blocks, and that along which, or nearly so, the greatest number have proceeded, is very approximately coincident with the directions of the striæ along the same line. A similar law holds with respect to other blocks which can be traced to their respective original sites. It may, therefore, be asserted as a law in this region, that the general or mean directions of transport are approximately coincident with the directions of the striæ.

If we refer to the analogous phenomena of Scotland, we find the general law which characterises them is exactly that above enunciated; but when we examine the details of this latter case, it appears that the general law is only approximately true, for the law of divergency does not accurately hold with reference to one general centre, but with reference to a number of particular centres. This I have proved in the memoir on the granitic blocks of the South Highlands of Scotland, inserted in the last Number of our Journal, with respect to the granitic nucleus of Ben Cruachan, and that of the group of mountains immediately on the west of the northern part of Ben Lomond. To complete our knowledge of the Scandinavian striæ, it is necessary to ascertain whether such particular centres are found also in the mountainous district of that

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region. This is one of the points to which I would especially direct the attention of observers.

So long as we restrict ourselves to the Highlands of Scotland, we easily recognise the circumstances which have determined the particular directions which the blocks have taken. They have followed the valleys which must have existed previously to their dispersion, wherever those valleys were sufficiently defined to govern the operation of the transporting agents. And this would appear also to have been the case in the more immediate vicinity of the Scandinavian chain. We may consider the striæ, then, to represent the general direction of transport, and we find them, as laid down on the map of M. Sefström, exactly coinciding with the directions of the river-valleys descending from the mountains. So perfect a coincidence leaves little doubt of the influence of the pre-existing valleys in the direction of transport. But as we recede from the mountainous district, even in the limited space between the Highlands and the eastern coast of Scotland, the configuration of the country no longer presents, in many parts, those determinate features which would necessarily give a definite direction to the masses transported across it; and how much more is this true with respect to the wide-spread plains of northern Russia and of northern Germany! And yet, in all these cases, the directions of the striæ obey, with wonderful regularity, the same law of divergency as those nearer to the central chain. We may easily understand how glaciers would descend down the mountain-valleys, and, after reaching the level of the sea, how the ice would float along the submarine continuation of the same valleys, leaving striæ along them, without the power of deviating from a fixed direction; but after having escaped from the valleys on the immediate flanks of the central mountains, what cause can have operated to drive forward through the more open sea these masses of ice, or the masses of other materials which may have been the striating and grooving agents, in the same continuous direction, and with such a force and determination that they could not be turned aside by the numerous projecting bosses of solid rock on which they have so effectively engraved the record of their transit? According to the hypothesis which we shall probably all be ready to adopt, the more elevated parts of the Scandinavian range must, at the period we are referring to, have formed an island, round which ordinary ocean-currents may possibly have passed in any direction; but the notion of such ordinary currents diverging in such various directions radiating from the central portion of this Scandinavian island, can only be spoken of as an absurdity. And yet no other force has ever been suggested, or is perhaps conceivable, except that of currents, as efficient to drive large icebergs or a mass of looser materials in a determinate direction, in defiance of numerous opposing obstacles. It appears to me, therefore, that we are driven to the alternative either of rejecting all theory on the subject, or

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of adopting that which would attribute these currents to *waves of elevation*, resulting from frequent, sudden, but not extensive vertical movements of the central range of elevated land; movements which we may conceive to have been thus repeated while the mean movement of the whole region was one either of gradual depression or of elevation.

And here I would make an observation which may not perhaps be without its theoretical value. Adopting this view of the subject, we may conceive the centres of the elevatory movements to have been different at different times, and consequently the directions of the corresponding currents produced by them to have been different, as in fact they would appear to have been from the different directions in which the transported matter has been driven from the same original site. But the movements which would send forth the greatest quantity of floating ice would be those which more immediately affected the line of coast; and the coast being deeply indented, as it must have been, by the present river-valleys when submerged, torrents would be simultaneously discharged from their mouths which would determine, in a material degree, the resulting current in the open sea; and since these valley-currents would necessarily have always the same directions, they would tend to impress approximately the same constant direction on the resulting ocean-current, whatever might be the precise centre of the elevatory movement. This influence, however, would, of course, be principally felt at points least remote from the then existing coasts.

When we pass to the great field of northern drift which the continent of North America presents to us, it is not perhaps without some feeling of disappointment that we find the directions of the striæ and those of transport without any distinct character of divergency either from local centres or from a general one. The observations described in Dr Bigsby's paper on the "Erratics of Canada," were made before the importance of striated and polished rocks had been recognised, or we should doubtless have obtained much valuable information respecting them from so careful an observer. We learn, however, from the American geologists, that the striæ preserve an approximate parallelism in a north-westerly and south-easterly direction over the north-eastern part of the North American continent, and that the erratic blocks and other transported matter have come in the same direction. In northern Europe, when the striating agents had quitted the Scandinavian mountains, they met with no other mountains of sufficient magnitude to impede their general course, or materially modify the directions of movement; but in America the striation, according to the American geologists, has been carried not only transversely but obliquely over some of their highest mountains, without material deviation from its normal direction, except along

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or near the bottoms of some of the valleys, in which cases the direction of the striæ nearly coincides with those of the valleys.

This coincidence of direction in the lower parts of the valleys is exactly what we should expect, and is accordant with the character of the like phenomena in Europe; and the persistency of transverse oblique directions in the striæ over the upper parts of elevated tracts presents no difficulty; for so long as the striating agent (as an iceberg) should only come in contact with those upper parts, its operations could not be influenced by the depths of the valleys below. But what takes place at intermediate heights between the bottoms of the valleys and the tops of the mountains? It is impossible to suppose, if the side of a mountain were striated in every part, that while the striæ at the bottom should be parallel to the lateral valley or axis of the mountain, and those at the top should be, for instance, perpendicular to it, the striæ at intermediate heights should not have some intermediate directions in passing from one extreme limit to the other. Careful observations ought to be made on this point. The height to which the striæ preserve their parallelism with the valleys below, and the distance from the tops of the higher ridges across which they preserve their transverse directions should be most carefully noted. Nor ought any geologist, in a delicate question of this kind, to trust to vague measurements and general impressions. Every direction ought to be carefully taken, and as carefully laid down on a good physical map, together with the dip and strike of the striated surface. The general configuration, too, of the immediate vicinity should be described, with reference to its probable influence on the motion of any mass to which the striæ may be attributable. Again, it has been said that in many cases the *lee side* and *storm side* of an elevated ridge are sometimes equally marked by striæ *transverse* to its direction. This seems entirely at variance with our observations on this side of the Atlantic, except in those cases in which the striæ are attributable to *local* action, in contradistinction to that more general action of such agents as masses of ice, for instance, driven in one direction over the whole region from NW. to SE. I have not hitherto been able to represent to myself the physical possibility of striæ on the lee side *remote from the top of the ridge*, having been produced by the general action just referred to. May they not have been more frequently due to local action than has been suspected? The glacial theories, on their first introduction, did not, I think, make so much impression on the minds of American as on those of European geologists, and many of the recorded observations of striated rocks were made, if I mistake not, under impressions very unfavourable to those theories. Let me not be thought by this remark to cast a reflection on American geologists—men to whom our science owes so much, and from whom it expects so much more in the

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noble field in which they are labouring; but we shall all do well, gentlemen, in learning to doubt the completeness of our observations on difficult and controverted points when made under the strong impressions of antecedent convictions. What I am especially anxious for is to see the American geologists resuming their observations in all possible detail on this interesting subject, and with candid reference to the different physical causes to which smoothed and striated rocks have been attributed. There are few phenomena more likely to elucidate the mixed and perplexing operations of the period to which they must be referred. In northern Europe M. Sefström has set us an admirable example, by his careful and exact manner of making his observations, and of mapping the results of them. There is still much room for following out similar observations in the Scandinavian regions. In our own islands, too, in Ireland, we have a field in which much yet remains to be done. The observations on these points by my friend Mr Griffith were made, as he has told me, a considerable time ago, and incidentally rather than as forming a leading object in his researches. It is not, therefore, to be expected that they should be sufficient to satisfy the present requirements of the science. If by these remarks, gentlemen, I should perchance lead any geologist to reflect on the geological importance of this subject, and to make and record his observations upon it with more than ordinary accuracy, I feel that I shall be attaining one of the best objects for the accomplishment of which an address of this kind may be rendered useful.

I shall now proceed to make a few observations on the arrangement of the materials which constitute the drift of Northern Europe. Though in many cases this arrangement seems very confused, as we might expect it to be, there does appear to be frequently a decided predominance of finer material in the lower, and of coarser material in the upper portion. The lower mass frequently consists of fine argillaceous and arenaceous sediment, sometimes mixed with rolled pebbles, and reposing immediately on the polished and striated rocks. Taking the whole area of deposition in Norway, Denmark, Sweden, Northern Russia, and Northern Germany, the materials above described constitute the great mass of the drift; and on this mass generally the large erratic blocks are superincumbent, though many blocks are also found imbedded within its mass. The submarine origin of the general mass is rendered unequivocal by the organic remains which it is found in various localities to contain.

The boundary of the area over which this enormous mass of detrital matter has been deposited proceeds from a point east of the White Sea towards the south-east, until it touches on one point only on the Ural Mountains, whence it proceeds south of Moscow to the Carpathian Mountains, and includes the whole of

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Northern Germany. Throughout Russia and Poland it is laid down in the map which accompanies the "Geology of Russia." Independently of its zigzag irregularities, it may be considered approximately as the circumference of a circle, having its centre near the northern extremity of the Gulf of Bothnia. A very large majority of the blocks dispersed over this immense area can be distinctly referred to their Scandinavian origin, thus shewing in a remarkable manner the centrifugal or radiating action already mentioned of the forces by which this dispersion has been effected.

The granite-boulders seem to have been in this, as in so many other cases, the best travellers. They constitute the greater part of the blocks in the external zone of the drift. But it is of more importance to remark, that whatever may be the nature of the blocks, they become almost universally smaller and more rounded as we approach the external boundary above indicated. This seems to me conclusive as to the nature of the transporting agency in this outer zone. I can conceive water alone to be capable of giving these characters to the transported materials. On the contrary, as we approach the central portion of this region of drift, we find the blocks of enormous size, perfectly angular, and not unfrequently imbedded in masses of fine drift, indicative of the absence, at the time of its deposition, of any violent currents capable of moving the blocks imbedded in it. In this we recognise the transport by floating ice. And again, on the central land, we recognise glaciers as the source of the floating ice, and the means of transporting large angular blocks from their original sites on the mountains to the level of the ocean.

You will not suppose, gentlemen, that, in stating these conclusions, I regard myself as opening new views to you. My object is merely to present the subject to you in a general but compendious form, in the hope that I may thus lead you to contemplate its various points collectively, and to see how much they are brought into harmony with each other by a distinct recognition of the three causes above mentioned, and a due allotment of the varied phenomena of the drift to their respective modes of transport.

The authors of the "Geology of Russia" consider the present boundary of the region of the drift in North-eastern Russia as indicating the approximate boundary of the glacial sea in that region during the drift-period, and this conclusion appears to me perfectly legitimate. They also consider the low, flat lands of Northern Asia to have been, about the same period, under the sea. In favour of this view, there appears to be the unequivocal, though not perhaps abundant, evidence of marine remains. There seems to be no evidence, however, of a submergence of this region approximating in depth to that of many parts of the European continent; the present low lands were probably covered only with

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shallow water. And hence we may conclude that Northern Asia was in a state of comparative repose during the period of much greater oscillation, and probably of more frequent and comparatively violent disturbance of the European area. Again, no traces of former glaciers have been detected on the Ural Mountains, or on the projecting headlands which run out to the northward from the high lands of Northern and Central Asia. This former absence of glaciers during our glacial period, in a region now so much colder than Europe, appears at first sight a great anomaly. It presents, however, no real difficulty, because those very causes which I believe to have produced the glacial cold of Europe would necessarily diminish the cold of Northern Asia, and more especially that portion of it immediately east of the Ural chain, as I have explained in my paper "On the Causes of Changes of Terrestrial Temperature." This effect would be due to the extension of the Atlantic Ocean to the eastward, so that the region of the Ural would become part of the western shores of the old continent, and would experience climatal influences similar, though far less in degree, to those now experienced in our own region. Hence what I have termed the *line of* 32° *F.* would be higher in North-western Asia than at present. On the other hand, the extension of the ocean to the eastward would lessen the great difference which now exists in Northern Asia between the summer and winter temperatures; and on this account the height of the *snow-line* above the line of 32° would be diminished. Consequently the absolute height of the snow-line would be increased by the first cause and diminished by the second, and would probably be not very different from its present height, though it might possibly be somewhat less. Now, since the configuration of the mountains was probably very nearly the same at the glacial epoch as now, the existence of glaciers upon them would depend upon the height of the snow-line; and, that height not being materially altered, there is no more reason why glaciers should have existed there at the more remote than at the present epoch; and at present we know that there are none in the Ural chain as far as the 70th degree of latitude,* and none on the mountains of Northern Asia descending nearly low enough to reach the level of the shallow sea, which we suppose to have covered the low lands of that region during the glacial period.

This former absence of glaciers, and the comparative repose of Northern Asia during our glacial epoch, are sufficient to account for what appears at first sight extremely anomalous—the fact, that while on the west of the Ural mountains we have a district covered with enormous erratic blocks, there is scarcely a single block to be found on the east of that chain at any distance from its original

* *Geology of Russia.*

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site, the whole mass of detrital matter, too, being very small, and principally referable to merely local causes.

I cannot quit this part of our subject without reminding you of the lucid manner in which the authors of the 'Geology of Russia' have pointed out how well the above state of Northern Asia accords with the supposed existence of Mammoths during the glacial epoch, and how happily Sir Charles Lyell and Professor Owen explained the capabilities of those animals to sustain the hardships of a cold climate. But before the publication of Dove's Map of Isothermal Lines, we had no adequate means of accurately estimating the effect of such conditions as those above assumed on the climate of North-western Asia. The extension of the Atlantic Ocean nearly to the foot of the Ural chain would heighten considerably the mean annual temperature of the neighbouring land, especially if the height of that chain was lower than at present, as Sir R. Murchison supposes it to have been at the period in question. But the great effect would consist in the lessening of the enormous existing difference between the summer and winter temperatures already alluded to. The winter temperature would, doubtless, be very much moderated: and, therefore, any difficulty of conceiving how great Pachyderms could exist through a Siberian winter is in a great degree removed. Again, a much more adequate reason is thus assigned for their subsequent disappearance from that region. The cause to which this fact has been attributed, is an increase of cold, arising from some additional elevation of the Ural chain, and a rise of the region in general to the amount of a few hundred feet. I believe it, however, to be certain that these causes alone could produce but little influence on the climate; but, if we unite with them the withdrawal of the ocean from the Ural chain within its present limits, we have an adequate cause for changing the climate from one much more equable than at present to the extreme of a continental one; from a climate in which the mammoth might exist, to one in which its existence during the winter would be no longer possible. This would seem to afford a very adequate cause for the disappearance of the mammoths from the Siberian region; why they should not still have sought a refuge in lands somewhat more southerly, which must still have been open to them, may be a question of more difficult solution.

With respect to the *order* of events connected with the glacial epoch, conclusions have sometimes been drawn which do not appear to me altogether warranted by the observed phenomena. The striated and polished rocks, as fixed rocks *in situ*, must necessarily be subjacent, where they exist, to the lowest beds of the drift, frequently consisting of fine argillaceous and arenaceous matter. It has been hence inferred that the process of striating and polishing these subjacent rocks must have been altogether anterior to the

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whole process of deposition of the finer matter, each of these processes occupying distinct and separate intervals of time. No one would, of course, suppose that the matter reposing on a given surface of striated rock could have been deposited there before that surface became striated; but the real question is, whether these two processes of striating and depositing were not going on simultaneously in the region generally, though not absolutely the same points. If the striæ be due, as some geologists have supposed, to detrital matter driven by a rapid current, the two processes must of necessity have been simultaneous, the one where the current was most rapid, the other where it was less so. Or if we refer the striæ in the lower and flatter regions of the area of the drift to floating ice, how was it that the icebergs and the currents which impelled them onwards bore no detrital matter at that time, and so much at a subsequent time? I conceive the two processes to have gone on simultaneously. No agency for the production of striated and polished surfaces has ever yet been suggested which would not almost necessarily be accompanied with the transport, and consequently with the deposition of detrital matter. Currents and small icebergs might deposit from time to time detrital matter on a given rock-surface, but the first iceberg that succeeded, large enough to reach down to that surface and grind over it, would clear away the detritus previously deposited upon it, and smooth and striate the rock itself. This might be repeated for a long period of time, during which the process of striating the projecting surfaces might be contemporaneous with that of permanent deposition at points almost immediately contiguous, but at lower levels. Finally, supposing a continued subsidence of the general area, the projecting striated bosses would sink below the reach of the icebergs, and the transport of matter still continuing, would become permanently covered up. As the general area re-emerged it would be subject to denudation, which might be expected to lay bare again some of the striated rocks, and leave others permanently covered with detrital matter as we now find them.

Again, with reference to the combined operations of floating ice and currents, it is not unworthy of remark that the former would necessarily deposit least of its freight, *cæteris paribus*, in its unimpeded motion over deeper waters, and a greater part in its impeded course over shallow bottoms. On the contrary, currents would deposit least on the shallow bottoms, where, *cæteris paribus*, their velocity would be greatest, and most in the deeper waters; and, moreover, it would be in these deeper waters that the finer matter would be deposited. Thus the existence of beds of finer and in many cases stratified deposits, having more tumultuous deposits, possibly both above and below them, as in some parts of North America, does not necessarily indicate a cessation in the more energetic action of the forces of dispersion, but may merely indicate

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deposition in a deeper sea. If also, large angular blocks from distant sites should be imbedded in this mass of finer matter, we see an additional indication of a deep sea, in which a floating iceberg would, perhaps, at distant intervals, drop a portion of its freight.

There is also a consideration connected with the process of transport by certain currents alone, which, with reference to our inferences as to the succession of events, is of some importance. I have mentioned it in my memoir "On the Granitic Blocks of the South Highlands of Scotland," which appears in the last Number of our Journal. Currents attending waves produced by sudden elevations, greater or less, are necessarily *transitory*, and each can only carry the materials it may transport to certain distances, depending, *cæteris paribus*, on the magnitudes of the component individual masses, the large blocks being carried but to small distances, and the smaller particles to much greater distances. Thus the first wave would produce a layer consisting of the larger blocks near their source and of fine detritus at the remoter distances. The second wave would produce a similar effect, and would also carry the blocks of the first wave to a somewhat greater distance, and so on for successive waves. The effect, then, of a succession of similar waves would be the formation, over the more remote parts of the area of deposition, of a bed of finer matter, in the *upper portion* of which would exist blocks rounded and waterworn by their transit. Thus we should have the phenomena of fine detrital matter below and blocks above, apparently referable to several successive periods of time, during the first of which one kind of agency should have transported the finer sediment, and during the second another and much more powerful agency should have transported the blocks and coarser detritus, while, in fact, the whole phenomena would be really referable to a repetition of precisely the same agency during the whole period of transport. That period, therefore, except in a limited sense, and not with reference to the whole area of transport, could not, in the case now supposed, be divided into two, but must be regarded as one single period.

I do not mean here to assert the opinion that the actual glacial period recognised by geologists was characterised by a uniform succession of exactly similar events producing erratic dispersion. There might be particular portions of that period in which accidental circumstances produced a greater or less prevalence of each particular mode of transport; but I am satisfied that some of the attempts which have been made to subdivide the glacial period have been made without due regard to such considerations as those which I have given above.

Let us now turn again to the drift of North America. The American geologists appear for the most part to recognise three distinct periods into which the whole period of the drift may be divided. The first period was one of the transport of blocks and

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coarse materials; the second one of tranquil deposition; and the third was again a period of transport of large blocks and coarser matter. This generalization appears to have been principally founded on the characters of the drift of Lake Champlain and that of the general valley of the St Lawrence, where the beds of the second period not only consist, in great part, of finer matter, but are also, in many instances, distinctly stratified, and filled with organic remains. But before we can adopt these subdivisions of the general period with reference to so many distinct modes of action of the transporting agencies, or of the different degrees of intensity with which they acted, it will be necessary to prove the above-mentioned succession of beds to be *general* and not merely *local*. If local, I should be disposed to refer the tranquil deposition of the fossiliferous and associated beds, partly at least, to the condition of a deeper submergence than at the periods of the transport of the coarser beds and blocks above and below the finer beds. I see no reason, in local facts of this kind, to infer that there were three distinct periods with reference to the intensity or mode of action of the dispersing forces. I may here observe that Dr Bigsby detected no evidence of this subdivision of the drift in the region which he examined further to the west.

Some of the American geologists appear to have entertained the opinion that the Mastodon existed in that region after the latest period of the drift, and seem to refer its final destruction to some upheaval of the American continent. It may be doubted, however, whether any evidence has been offered of the existence of that animal later than the latest drift in which its remains are found; nor do I understand how the cause just assigned could effect its final extinction. If, however, we admit the submergence of that continent to the extent which many geologists are now disposed to admit, there can be no difficulty in explaining the extinction of any of the great pachyderms which might have previously inhabited that region.*

II. *On the Causes of Change in the Earth's Superficial Temperature.*

The next paper to which I shall call your attention, although not directly on the subject of the drift, may be considered as closely associated with it, one of its principal objects being to account for the peculiar climatal conditions of the glacial period—that period to which geologists now universally refer the general phenomena of drift. I allude to the paper which I have myself brought re-

* To this account of Drift, there follow in the Address, numerous details regarding the drift of North America, Europe, and Australia, for which we have no spare space at present. The alluvial gold of the Diggings—the *curse of our time*—is noticed as to *priority* of discovery. Geologists appear to have had little to say in this business—and so much the better.—*Editor.*

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cently before you, "*On the Causes of Change in the Earth's Superficial Temperature*." You will recollect that, until very recently, the only change of climate which had been recognised by geologists as having taken place during the earth's geological history was one from a higher to a lower temperature, and, for those who believed in the *primitive heat* of the globe, that heat afforded one obvious cause for this higher temperature at remote geological epochs. When, however, an examination of the phenomena of the glacial epoch rendered it necessary to recognise a change of climate in our own region of the globe from a lower temperature during that period to a higher subsequent temperature, new conditions were added to the problem, which rendered the cause formerly assigned manifestly inadequate for its solution. Two other causes, however, had been previously suggested, which might possibly account, not only for a change from a higher to a lower superficial terrestrial temperature, but also for oscillatory changes. One of these assigned causes rested on the hypotheses of motion of the whole solar system in space, and the variable temperature of the different regions through which it might thus pass; the other cause assigned was the influence of different configurations of land and sea on the climatal state of particular portions of the earth's surface. Thus of the three causes above alluded to, speaking of them with reference to the earth's surface, one was internal, another external, and the third superficial. No attempt, however, had been made to examine the efficiency of these different causes to account for all the phenomena which may be referable to them. It was to remedy this defect that I undertook the investigations contained in the paper of which I am speaking.

Assuming the primitive temperature of the globe to have been very much greater than at present, there is manifestly no difficulty in accounting for any higher superficial temperature than the present, at past epochs, provided those epochs be sufficiently remote. They must, however, be exceedingly remote to enable us thus to account for a variation of temperature which should sensibly affect the climatal conditions in any part of the earth. The terrestrial temperature, to the depth of about 70 feet, varies with the progress of the seasons, the variation becoming less as the depth is greater, until, at about the depth just mentioned, it is no longer sensible, so that a thermometer placed there would indicate a constant temperature during the whole year. A second thermometer at a greater depth would also indicate a constant temperature throughout the year, but higher than that indicated by the preceding one. If this second thermometer were placed at a still greater depth, it would indicate a still higher constant temperature; and the increase of temperature between the two thermometers would be proportional to the distance between them, *i.e.*, the temperature in descending below the first thermometer would increase at a constant uniform rate.

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Again, if the cooling of the earth were to continue for an indefinite period of time, assuming the temperature of external space, the sun, and the earth's atmosphere, to remain as at present, the superficial temperature would approximate indefinitely near to a certain limit. The difference between that limit and the earth's present superficial temperature is the effect due to the remains of the primitive heat. Now theory gives us a simple relation between the amount of this effect and the rate of increase above-mentioned as we descend below the earth's surface.* Consequently, knowing the one, we can immediately determine the other, and thus, having ascertained the above rate of increase, we know the amount of superficial temperature which is now due to the earth's primeval heat, assuming always that heat to be the cause of the existing internal temperature of the globe. This amount is thus proved not to exceed about the $\frac{1}{30}$th of a centesimal degree, so nearly has the earth's superficial temperature approximated to that ultimate limit beyond which it could never descend, supposing external conditions to remain the same. It was calculated by Poisson that, to reduce the superficial temperature by one half of the above amount, or $\frac{1}{60}$th of a centesimal degree, it would require the enormous period of one hundred thousand millions of years. It would, doubtless, require us to go back into the past some such immense period as this to arrive at the epoch when the superficial temperature should have exceeded its present amount by even one or two degrees. At the same time the rate of increase of temperature in descending beneath the surface would be much more rapid than at present. If the superficial temperature amounted to 2° C. above its ultimate limit, instead of being $\frac{1}{30}$th of a degree, the rate at which the temperature would increase in descending would be about sixty times as great as at present, *i. e.*, there would be an increase of 1° C. for little more than one foot of depth.

It must be recollected that this state of terrestrial temperature, if due to the cause we are considering, could only have existed at times which, even in a geological sense, must have been extremely remote. The important peculiarity of this state of the earth would seem to consist in the simultaneous existence of a superficial temperature, and therefore of climatal conditions, very nearly the same as at present, with an internal temperature at the depth of a few hundred feet and upwards, immensely greater than at present. If we suppose the process of sedimentary deposition to have been then going on, we may understand how great an effect might be produced by this internal temperature in the metamorphism of the earlier sedimentary beds.

* If f denote the excess of the present superficial temperature above the final limit to which the temperature would descend in an indefinite period of time, and g the rate of increase of temperature mentioned in the text, we have $\frac{f}{g}=b$, where b is nearly equal to unity.

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The temperature of any point in stellar space is that which would be indicated by a thermometer at that point receiving the heat radiating from all the stars composing the universe. The temperature of all bodies must necessarily be affected by this radiation, and in different degrees, according to the positions in space which they may occupy. Hence Poisson was led to adopt the notion that the actual temperature of the earth, whether superficial or internal, is due to the circumstance of the solar system having passed through a warmer region than that which it now occupies, in the course of that motion by which astronomers generally believe it to be constantly moving from one part of space to another. What may have been the possible effect of this cause in the lapse of indefinite time, it is impossible to say; but I cannot understand how it could be very considerable without a totally different distribution of the group of stars to which the sun should belong, or the near approach of the solar system to some individual star. The latter hypothesis, however, would be inconsistent with the integrity of the solar system as it now exists, if we suppose the proximity to any single star to become such as to produce any material modification of terrestrial climate; and perhaps it may be difficult to conceive how the first hypothesis should escape a similar objection. At all events, it may be regarded as certain, that according to neither of these hypotheses can any considerable effects have been produced by this cause on terrestrial temperature within the later tertiary period, and that we cannot thus account for the cold of the glacial epoch.

In considering the influence of the third cause,—that of the configuration of land and sea,—I have endeavoured to ascertain approximately what would be the climatal conditions, more especially in western Europe, in the four following hypothetical cases :—

1. The configuration of land and sea the same as at present, but without the Gulf Stream.

2. The Gulf Stream the same as at present, except that its progress into the North Sea is supposed to be arrested by a barrier of land, extending from the North of Scotland to Iceland, and thence to the coast of Greenland.

3. The basin of the Atlantic from the Tropic to the North Sea converted into land, uniting the old and new continents.

4. Large portions of the continents of Europe and North America submerged beneath the surface of the ocean, and the Gulf Stream directed into some other course.

By a study of Dove's admirable Map of Isothermal Lines, we easily recognise the masses of land in the northern parts of the old and new continents, and the Gulf Stream as the principal causes of the abnormal forms of the isothermals in the higher latitudes of the northern hemisphere. In like manner the irre-

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gular forms of the known isothermals of the southern hemisphere, extending to about the latitude of 50°, may be seen to be attributable to similar causes, more especially, perhaps, in this case to well-known ocean-currents; and a knowledge of these causes enables us to draw the isothermals in such hypothetical cases as those above stated with approximate accuracy. This is what I have first attempted to do in the memoir before us.

Taking the first case, I arrive at the results embodied in the following table:—

	At present, with the Gulf Stream.	Difference.	Without the Gulf-Stream.	Difference.
The Alps.				
Temperature for January, . . .	38° F.	35°	34° F.	39°
... ... July, ...	73		73	
Mean annual temperature, . . .	55·5		53·5	
Snowdon.				
Temperature for January, . . .	38 F.	23	23 F.	38
... ... July, ...	61		61	
Mean annual temperature, . . .	49·5		42	
Northern Extremity of Scotland.				
Temperature for January, . . .	36·5 F.	19·5	12 F.	44
... ... July, ...	56		56	
Mean annual temperature, . . .	46·25		34	
Centre of Iceland.				
Temperature for January, . . .	30 F.	22	−4 F.	50
... ... July, ...	52		46	
Mean, ...	41		21	
Mean annual temperature,* . .	39			

In the case in which the Gulf Stream is supposed to exist, but its progress into the North Sea to be arrested by a continuous barrier of land, I have shewn that the winter temperature of the coast of Iceland would probably be increased 6° or 7° F., and that the January isothermal would probably run nearly north and south from Iceland to the latitude of Central France. You will recollect that a former littoral or sub-littoral communication between the western coasts of Europe and the eastern coasts of America is rendered probable by a certain community of specific forms in those localities. My object in considering the effect of

* This is deduced from the mean of the monthly temperatures. The mean annual temperatures above given for the other cases are almost identical with those deduced from the monthly temperatures. The discrepancy of 3° in the case of Iceland may be attributed to local peculiarities.

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the configuration of land above-mentioned, is to determine how far it might afford this littoral communication with a temperature of the ocean sufficiently high to admit of the dissemination along it of the species alluded to.

The next case is that in which the basin of the Atlantic should be converted into dry land, so as to unite the old and new continents. This would give to our own region the extreme continental climate of Northern and Central Asia. According to my estimate, we should then have for Snowdon,

Temperature of January . .	7° F.	Diff. 73°·5.
„ July	66°·5	
Mean annual temperature .	29°·75	

The summer temperature would be increased 5°·5 F., but the winter temperature would be reduced 45°, and the mean annual temperature 20°.

In discussing the fourth case, in which the Gulf Stream is not supposed to exist on our own shores, and a great part of Europe is assumed to be submerged beneath the ocean, I have shewn that the mean annual temperature would be very nearly the same in western Europe and in the latitude of Snowdon, as in the case above considered of simply the absence of the Gulf Stream. The conditions under which the Welsh and Irish mountains would be placed, supposing them extant above the sea while the neighbouring region was submerged, would be very similar to the existing conditions of the Falkland Islands, and the island of S. Georgia; and a comparison with these islands leads me to conclude that the estimate above given of the mean annual temperature of Snowdon (42° F.) is two or three degrees too high. I have considered 39° or 40° F. to be a nearer estimate. In fact, a great part of the misconception which has existed respecting the possible past temperature of this region has arisen from our regarding its present temperature as the normal temperature for our own latitude, and that of places like the island of S. Georgia, in corresponding south latitudes, as the abnormal temperature; whereas the exact reverse of this is the actual case.

Having determined the positions of the isothermal lines for any particular hypothetical case, we can determine, for that case, the mean annual temperature at any assigned place. The object which I have next proposed to myself in this paper is more especially to determine the conditions under which glaciers would exist in those parts of western Europe where traces of their former existence have been observed. The principle on which I have proceeded is easily explained. The *snow-line* is that line on the side of a mountain which forms the highest limit to which the boundary of the snow ascends during the year. It bears an important relation to all glaciers, being that limit below which the

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glacier receives no permanent superficial increase. Below this limit the destructive begin to prevail over the productive agencies. The distance to which the glacier descends below it depends on local circumstances; but we find that nearly all glaciers, large enough to be considered of the first order, descend to levels lower than the snow-line by an amount varying from about 4000 to 5000 feet. In smaller glaciers the descent is proportionally less. Again, the snow-line bears certain relations to the line which I have defined as the *line of* 32° F., or that along which the mean annual temperature is equal to 32° F. At certain places in sufficiently high latitudes this line will lie at the level of the sea. In lower latitudes it will lie at higher levels, and in still higher latitudes the mean annual temperature will be less than 32°. It is found that at the equator the snow-line lies about 1000 feet below the line of 32°, while in the higher north latitudes it lies above the latter line, the vertical distance between them being very variable. A continental climate, in which the atmosphere contains comparatively little moisture, and the variation from summer to winter temperature is very great, is favourable to a relatively high position of the snow-line; while an insular climate, in which the quantity of moisture is comparatively large, and the annual variation of temperature comparatively small, superinduces a relatively low position of this line. Thus in the north-eastern part of Asia the snow-line is probably from 4000 to 6000 feet above the line of 32°, while in Iceland its height above the latter line does not exceed a few hundred feet. A knowledge of these facts enables us to estimate approximately the vertical distance between these lines in any proposed hypothetical case. To estimate the absolute height of the snow-line above the sea-level, we have only then to calculate the height of the line of 32° at the place proposed. For this purpose we must estimate the mean annual temperature there by means of the isothermal line passing through the place, and then calculate the vertical height to which we must ascend to reach the point at which the mean annual temperature is equal to 32°; and to effect this we must know the height which corresponds to a decrease of temperature of 1°. Humboldt and others have shewn that this height may be taken as varying from about 320 to 350 feet in ascending steep mountains, or vertically in a balloon; but Humboldt has also shewn, from his own observations, that, in an ascent presenting a succession of high and extensive table-lands, the increase of height for each degree may amount to 450 or 500 feet. This is an important distinction.

In this manner, then, the height of the snow-line above the sea level can be estimated at any proposed place, with any hypothetical distribution of land and sea. If a mountain rise a few hundred feet at least above the snow-line, and the configuration

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of its summit to be favourable, glaciers will be formed upon it, of which the magnitude will depend on circumstances. If large, we might expect them to descend 4000 or 5000 feet below the snow-line, and to a distance proportionally less when the glaciers should be small.

As an example, we may take Snowdon, in the case in which the Gulf Stream is supposed to be absent from the shores of western Europe, and a large portion of that continent submerged beneath the ocean. I have shewn that the temperature of Snowdon would probably not exceed 39° or 40° F. Assume it 39°. Taking a decrease of temperature of 1° in ascending 320 feet, the height of the line of 32° would be 2240 feet. The climate would be entirely an insular one, and therefore the height of the snow-line would probably not exceed that of the line of 32° by more than 200 or 300 feet. If we suppose this additional height to be 260 feet, the absolute height of the snow-line would be 2500 feet, or 1000 feet less than that of the present summit of the mountain. If we assume the mountain to subside 400 or 500 feet with the surrounding region, it would still rise 500 or 600 feet above the snow-line, a height sufficient to admit of the formation of glaciers, which might descend to the level of the sea. If, in addition to the hypothesis of the absence of the Gulf Stream, we adopt also that of a cold current from the north, the mean annual temperature might be reduced 3° or 4° F. lower than above supposed, which would reduce the height of the snow-line to 1200 or 1500 feet. This would admit of the formation of glaciers, not only on Snowdon, but also on the lower mountains of Ireland. And I may here remark, that if we can thus account satisfactorily for climatal conditions consistent with the existence of glaciers in the south-west of Ireland, the subject presents no difficulty with reference to any other part of western Europe, in which we observe the traces of glacial action.

For the application of the same method of investigation to the other hypothetical cases of the distribution of land and sea, I must refer to the memoir of which I am speaking. I have selected the above case, not only because it seemed best calculated to elucidate the subject, but also because I consider it that which has far the highest claim to our acceptance as the real one of the glacial epoch. It involves, as we have seen, the absence of the Gulf Stream as an essential condition, the explanation which it affords of the existence of ancient glaciers being rendered more complete by the supposition of a cold current from the north. On these points it remains for me to say a few words.

I need scarcely remind you that the evidence of the submergence of a very large portion of the North American continent during the Drift period is similar to that for the submergence of Europe. A subsidence of less than 2000 feet would render the ocean continuous from the Apalachian chain, on the east, to the Rocky Mountains on

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the west; and there seems reason to believe that the subsidence may possibly have attained to a considerably greater amount. Now it is manifest that the Gulf Stream is reflected in a north-easterly direction across the Atlantic, by the continent of North America, which arrests the north-westerly course by which the current reaches the Gulf of Mexico. But when that continent was submerged, as above supposed, the current would necessarily continue its north-westerly course, and probably along the foot of the Rocky Mountains directly into the Arctic Sea. This is the manner in which I conceive the Gulf Stream to have been diverted from the shores of western Europe. This diversion of the current is not to be regarded as a mere hypothesis adopted to account for any particular fact, but as a necessary consequence of that submergence of the North American continent.

Again, if this enormous current discharged itself into the Arctic Sea, it would seem extremely improbable that it should not give rise to some great determinate counter-current *out* of that sea. Now it appears highly probable that a considerable tract of land must have existed at the period of which we are speaking in the present region of north-eastern America and Greenland. If this were the case, the only practicable outlet for a great current from the Arctic Sea would be across the submerged portion of northern Europe, or along the present North Sea, between Greenland and Norway; for the passage through Behring's Straits, even with a considerable subsidence of the land on either side, would be neither sufficiently wide nor deep to form a considerable outlet. Under such circumstances, it would scarcely seem more necessary that the Gulf Stream should hold its original north-westerly course over the submerged continent of America, than that it should complete its circuit by passing through the Arctic Sea, and returning to the Atlantic across the submerged land of Europe, as it now completes a more circumscribed circuit by being constrained to pass along the northern portion of the Atlantic itself.

The effect of this diversion of the Gulf Stream from its present course, would not be less remarkable in elevating the temperature of the northern shores of America and Asia, than in reducing that of western Europe. I have shown that the mean annual temperature of Iceland is increased 18° or 20° F., and the January temperature 34°, by the influence of this important current. Now the distance from the Gulf of Mexico to Behring's Straits is very little greater than the distance between that gulf and Iceland, and the passage of the stream along the flanks of the Rocky Mountains would be more direct, and probably less impeded by counter-currents than it is at present in its transatlantic course. There can be no reasonable doubt, therefore, of its raising the temperature of the north-western coast of America, from the Mackenzie River to Behring's Straits, by an amount at least equal to that by which

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it now elevates the temperature of Iceland. Further, it is highly probable that the principal course of the current in the Arctic Sea would not be far from the coasts of northern Asia, the temperature of which would thus be affected in a manner similar to that of the coast of America eastward of Behring's Straits, although in a smaller degree for greater distances on the west of the Straits. The temperature of winter immediately east of the Ural Mountains would also be considerably moderated, as already stated, by the extension of the European sea towards their western flanks. The climate of the low lands of northern Asia would thus differ from the present climate of that region, as much as the existing climate of the western coast of Norway differs from that which would desolate that region in the absence of the Gulf Stream.

According to this view of the subject, the former existence in northern Asia of the immense numbers of large mammalia indicated by the abundance of their fossil remains no longer presents the slightest difficulty; and the theory receives a still further confirmation from an observation made by Sir John Richardson in his "Arctic Searching Expedition,"* just published. The author observes—"The existence of these numerous testimonials of an ancient fauna is suggestive of many curious speculations, and geologists seem hitherto to have failed in explaining the circumstances under which accumulations so vast could occur in such high latitudes. The difficulty is increased when we consider that these bones have not been detected to the east of the Rocky Mountains in high latitudes." This increased difficulty, however, is at once removed by the theory now proposed; for the region in which these remains are not found must either have been covered with the waters of the ocean to the foot of the Rocky Mountains at the period when these mammalia occupied the region to the westward, or, if land existed on the north-east of the present American continent, it was probably too cold to be inhabited by them. Their disappearance from the country bounding the Arctic Sea, from the Rocky Mountains to the Ural, would be the consequence of the withdrawal of the Gulf Stream from the more eastern, and of the European ocean from the more westerly portion of that region. Fossil plants, also, belonging to a comparatively warm climate, have been found east of the Rocky Mountains, on the coast of the North Sea; and extensive beds of lignite exist along the eastern flank of those mountains. So far as these phenomena may be of pleistocene origin, they may be at once accounted for by this theory. Its more complete verification must, however, be left to future observation. It will not fail, I hope, to attract the attention of American geologists.

* Vol. ii., p. 210.

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III. *Doctrine of Progression with respect to Animate Beings.*

The discovery in a single year of so much incontrovertible evidence of the extension of reptilian remains into some of the lowest fossiliferous strata is very remarkable, and calculated to add greatly to the interest of the discussion of the theory of the progression of organic forms during successive geological epochs, which has lately occupied the minds of geologists.*

It will be recollected that the advocates of the doctrine of progression, as well as the distinguished geologist who has been their principal opponent, distinctly repudiate the idea of the *progression* here spoken of involving any notion of the transmutation of species, a theory to which, as you well know, they have been most strongly opposed. They merely assert their belief that there has been upon the whole "a gradual ascent towards a higher type of being,"† from the earliest to the latest geological periods. They would also contend for a certain progression in the physical state of our planet, "a gradual improvement in the style and character of the dwelling-place of organised beings."‡ These are the propositions which your late distinguished President combated in the two addresses which he delivered from this chair.

It would be absurd to contend that either the first of these propositions or its negative have been yet established by demonstrative evidence. The advocate of progression can only reason on the confessedly imperfect evidence which we at present possess respecting the extent and variety of organic beings which existed at the earliest geological period to which we can refer the organic remains with which we are acquainted; and so far as his opponent proves to us more clearly the incompleteness of this evidence, and thus inspires us with due caution in forming our opinions, he renders service to the sound progress of speculative views on the subject. But if he goes further than this, and asserts the truth of the opposite proposition, he places himself in a position at least as untenable as that which he combats. If land existed when the earth first became the abode of animate beings, it would seem probable that animals adapted for such a dwelling-place should have been then created, and possibly in much greater numbers than the organic remains of the earlier geological periods might at present seem to indicate; nor need we be surprised at any

* The discovery of reptilian remains, alluded to by Mr Hopkins, refers to the impression of a reptile in the so-called Devonian sandstone of Elgin, and of reptilian footprints in the Potsdam sandstone of North America. We may remark, in regard to these statements, that the formation at Elgin has not been proved to be Devonian, and that the footprints in the Potsdam sandstone Professor Owen now considers as crustacean, not reptilian.—*Editor.*

† Prof. Sedgwick—"Discourse on the studies of Cambridge," Preface, p. cliv.

‡ Mr Hugh Miller—"Footprints of the Creator."

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future discoveries tending to support this view. Every fresh discovery like those above-mentioned brings us a step nearer to that ultimate limit to which our evidence can finally hereafter attain; but even if we make many more such additional steps, we must still be cautious how we adopt the opinion that that limit will indicate an absolute equality between the organisation of the earliest created beings and those which now exist—exclusive, I mean, of man, whose recent introduction on the earth is admitted equally by the contending parties. It is not here my purpose to advocate the one view or the other in mere special reference to organised beings, or to analyse the evidence which has been adduced with so much ability both in favour of the doctrine of progression and in opposition to it; but to insist on that philosophic caution and reserve which may leave us unshackled in our future speculations, and free to modify our opinions so far as future evidence may call upon us to do so.

IV. *Doctrine of Progression with respect to Inanimate Matter.*

So long as we restrict our speculations on the question of progression to organic life, we are in no danger of adopting conclusions inconsistent with what we know of the operation of natural causes, because we are ignorant of those laws by which the succession of organic life has been regulated since its introduction on the earth. Progression in organic structure, or the entire absence of it, may, for aught we know, be perfectly consistent with those laws. But the operations of nature have revealed much more to us respecting the laws which have been appointed for the government of the inorganic world, and it becomes us to examine how far these laws may be consistent with the doctrine of non-progression in its application to the *inorganic* matter of our planet, or how far they indicate a necessary tendency, in the language above-quoted, to "a gradual improvement in the style and character of the dwelling-place of organised beings."

And here I would remark, that the doctrine of non-progression, in the sense in which I use the term, is independent of that theory which would attribute all geological phenomena to causes acting with no greater intensity than those of which we now witness the operation around us. By *progression*, as applied to the inorganic matter composing our planet, I understand a change, continuous or discontinuous, by which that matter has passed, in the long process of time, from a primitive to its present condition, and may still pass to some ultimate and different state, a change by which, regarded in more especial reference to the question before us, the surface of the earth has been rendered more fit for the habitation of the higher orders of organised beings. This permanent change of state may have been effected or accompanied

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by either *paroxysmal* or gradual and uniform action of the forces which have modified the earth's surface. By *non-progression* I understand, not the absence of *periodically recurring* changes, but of that *permanent* change above mentioned. The periodical changes in this case, equally with the permanent changes of the former case, may have been produced or accompanied by either paroxysmal or uniform action. The *theory of non-progression* is essentially different from the *theory of uniformity;* and thus while we might allow the justice of the appeal to the Alps, made by my distinguished predecessor in this chair, in proof of non-progression as indicated by an apparent equality of intensity in the more recent and the more remote geological action, we might reject the appeal if made to prove that the forces which elevated the Alps were of no greater intensity than those which have been in action during the historic period.

It is not then, I conceive, to the phenomena of elevation, or of denudation and deposition, as indicating more or less of paroxysmal or tranquil action, that we must look for any demonstrative evidence to decide the question before us. But there is one most important agent which has doubtless been most active, not only in producing the phenomena of elevation, but also in modifying the characters of the inorganic matter composing the crust of the globe, and it is extremely difficult to conceive how the activity of that agent can have consisted with non-progression. The agent I speak of is *heat.* I assume the truth of the simple proposition, that *if a mass of matter, such, for instance, as the earth with its waters and its atmosphere, be placed in space of which the temperature is lower than its own, it will necessarily lose a portion of its heat by radiation, until its temperature ultimately approximates to that of the circumambient space, unless this reduction of temperature be prevented by the continued generation of heat.* If there be any propositions in experimental science which may be deemed incontrovertible, this, I conceive, is one of them. Now we know that the interior temperature of the earth is higher than that of its surface; and, in order that this state of terrestrial temperature may be consistent with non-progression, it must either be a permanent one, or must belong to a series of changes recurring periodically, but producing no permanent change or progression from a higher primitive to a lower ultimate temperature. If the present temperature be permanent, it must be maintained by some cause constantly acting within the earth, and generating a quantity of heat exactly equal to that which is lost by radiation into surrounding space. No external cause, such as solar or stellar radiation, could produce an absolutely constant, stationary temperature, which should increase in descending beneath the earth's surface. Chemical action might produce this effect, possibly, for a *finite* time, but philosophers, I imagine, would no more believe that or

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any other internal cause capable of producing such an effect for an *infinite* time, than they would believe in perpetual motion, in the ordinary sense of the expression. I cannot conceive, therefore, the present state of terrestrial temperature to be a *permanent* state. Can it belong to a perpetually recurring series of changes? I would reply, that no internal cause could account for any such *infinite* recurrence, more than for unlimited permanence of temperature. Such infinite recurrence could only be attributed to the external causes—solar and stellar radiation. If to the former, the quantity of heat radiating from the sun must be subject to enormous periodical changes, but still without permanent diminution; if to the latter, it might be attributed either to similar periodical changes in the radiation of the stars, or more probably to a change in the position of the solar system with reference to them, as I have explained in my paper on Terrestrial Temperature. But we shall probably all agree in regarding such hypotheses as extremely unsatisfactory, and utterly unfit to be made the foundation on which a great speculative theory may rest. But however unsatisfactory they may be, I repeat that we have no other alternative but that of adopting one of them, consistent with the most fundamental properties of heat, if we maintain the theory of non-progression in the strict sense in which I have used the term. And, having placed the theory in this point of view, I might leave it there without venturing into those speculations which assume the properties of the matter constituting the stellar universe to be the same as those which characterise the matter of our planet. Views founded on such assumptions ought to be advanced with diffidence, and held with cautious reserve; but if, with such reservation, we assume the sun and the stars to have the same properties as our own planet, with respect to the generation and emission of heat, we must conclude that those bodies must be subject to permanent changes of temperature, as well as the earth itself, from the effect of radiation. In such case even solar and stellar radiation must necessarily fail to preserve the earth from that permanent change of temperature which would constitute essentially a state of progression. In fact, adopting the assumption just stated respecting the nature of the sun and stars, and reasoning from all we know of the properties of matter and of heat, I am unable in any manner to recognise the seal and impress of eternity stamped on the physical universe, regarded as subjected to those laws alone by which we conceive it at present to be governed.

The rejection of the doctrine of progression, both with respect to animate beings and inanimate matter, would seem to lead almost necessarily to the opinion, that the sequence of periodical changes, similar to those which have happened within the period of which we can trace the geological history, has been of infinite duration. It would appear to involve the rejection of the notion of a *begin-*

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ning of the actual physical condition of our planet. If not, the earth must have been created at once, at some finite distance of time, as fit a dwelling-place for organic beings, as it has been rendered, according to the theory of progression, only by a long series of superficial operations. In other words, phenomena must have existed as the immediate act of creation, and anterior to the operation of physical causes, which it is the very essence of geology to account for by reference to those causes. This would be to sap the foundations on which alone geology can rest as a physical science.

I would again, gentlemen, carefully remind you that I have been discussing these theories with reference to my own definitions of the terms which designate them, and which others may not have accepted in the same rigorous sense. Still I believe that, if they are to bear a determinate meaning, they must ultimately be received in the sense which I have assigned to them. In that sense, leaving the question entirely open respecting the organic creation to be decided by future research, I feel it impossible to adopt any other view than that of progressive development of inorganic matter from some primitive to its present state. I have already remarked that we do not know sufficient of the laws which may regulate the succession of organic life on our planet, to assert that the one of these theories or the other is most consistent with these laws; but, with respect to inorganic matter, the theories of uniformity and of non-progression appear to me incompatible with our most certain knowledge of the properties of heat—that ever-active agent in the work of terrestrial transformation.

It is far from my purpose to enter into a discussion of the evidence which might be deduced from recognised geological phenomena in favour of the theory of progression, but merely to insist on that which depends on the most immediate and simple inferences from the properties of heat. And here it should be remarked, that this argument cannot be refuted by any reasoning which may appear to establish an *approximate* general uniformity or non-progression in the character of geological phenomena since the earliest geological epochs, because the progressive refrigeration of the earth from some high temperature to its present temperature is perfectly consistent with such approximate uniformity or non-progression for enormous periods of time. Climatal conditions, for instance, may, consistently with the earth's continual refrigeration, have remained sensibly unaffected by the internal heat, as I have elsewhere explained, for millions of centuries; and the very theory which tells us that these conditions can never be sensibly altered in all future time (external circumstances remaining the same) essentially involves the hypothesis of progressive change towards an ultimate limit.

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Volcanoes in the Bay of Bengal, &c. By Dr BUIST of Bombay.

(Continued from vol. lii. p. 352.)

The volcanic forms the terminal point of Southern Arabia, where the shore, after having inclined gently from Ras-el-Hudd, 21° north lat., at the entrance of the Persian Gulf, to 12°, stretches almost due west, till it turns up the Red Sea. At no great distance of time it has obviously been an island, and is now connected with the mainland by a low sandy spit four miles long, and half a mile across, only a few feet above high-water mark: the whole shore, indeed, consists of sandy downs or swamps, only a little above the level of the sea, and wearing the aspect of recent emergence. The peninsula itself is an irregular oval, five miles in its greater, and three in its lesser diameter. There are numerous little headlands with sandy bays between, all around it. There is at the head of each little bay, and on several points of the shore besides a level expanse of rolled gravel and sea-shells, evidently an old sea margin, brought to light by the same upheaval that converted the island into a peninsula, and raised the isthmus above the level of the sea. The rocks themselves are all lavas of various descriptions, more or less vesicular, and the volcano affords a vast diversity of igneous minerals. There seem to have been from time to time a number of craters in the mountain, one of very considerable magnitude beyond the coal depôt betwixt Ras Moorbut and Ras Tar Shagan, having been blown outwards, and now remains as a valley ascending from the sea. The edge of the principal crater is near the centre of the peninsula. The crater itself occupies the eastern half. It is exceedingly well-defined indeed, and at once indicates its origin to the spectator. It is about one-and-a-half mile in diameter, and is nearly circular, affording a circuit of five miles. Of this, half-a-mile has been blown out right down to the level of the sea. The bottom of the crater, on which stands the town of Aden and the British cantonments, is covered with a bed of rolled gravel and sea-shells, proving that there has been no trace of eruption since the last general upheaval, which produced the sea-beach all along these shores, but which is still believed to have been

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within the human, perhaps even the historic, period.* The Shum Shum range, which forms about half the wall of the crater, reaches an altitude of about 1760 feet. There is a huge crack or slip which cuts above a third off the eastern side of the volcano, and through a portion of this, constituting a narrow gorge or pass ten feet wide, and twenty or thirty high, the road from Steamer Point enters the crater and leads to the cantonments. Dr J. P. Malcolmson supposes this to have been the remains of the latest great eruption, of which the effects are chiefly manifest on the table-land on the eastern buttress of Shum Shum. By this the ancient crater was shattered nearly through its centre from the northern to the southern pass, breaking into pieces, and separating the whole of the eastern side, of the edge of which Sheera Island is a fragment; and in these views I concur. (Lond. As. Trans., 1846). On the one side of this which remains, the wall of the crater subsides from 1700 to 600 feet, and then breaks away altogether. The reft probably occurred when the side of the crater was blown out and demolished. The walls of the crater, as now existing, when seen from the cantonments, present the most magnificent view that can be imagined; one semicircular precipice, five miles in circuit, ascends some 1776 feet from the plain. It is, in most cases, perpendicular. The cliff is of a rusty dark-brown colour, and full of caverns and recesses, like the altar-screen of a Gothic cathedral. Great streams of lava may be observed from point to point, as if the fiery cataract had been arrested in its progress and congealed as it flowed from the lesser rents of the principal crater. On many parts of the rock, 500 feet above the level of the sea,—to the level of Shum Shum so far as I know, but the altitude just named is all to which I have examined it,—great masses of volcanic ashes are strewed amongst the crevices of the rocks, these generally abounding, as does the surface all around, with sea-shells in a state of great decay, to all appearance borne up by the volcano on its last emergence from the sea.

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* See Report of the Society of Civil Engineers, May 20, 1851. Also Miss Fanny Corbeaux's Letters; Athenæum, June 28, 1851.

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The minerals found at Aden are very numerous. We have almost every variety of lava, compact, earthy, vesicular, amygdaloidal, and porphyritic; obsidian in all its forms, from dull coarse green and blush green to beautiful jet black. Pumice is found, but it is not plentiful. It is mostly of a dark reddish-brown colour, and is heavier and more coarse in its texture than the mineral of commerce, and less suited for the finer purposes of the polisher. Sulphur is sometimes found, but rarely. Rock crystals in veins next, and crusts are very plentiful everywhere. On many parts of the volcano chalcedony in various forms abounds; in some cases it appears in thin crusts, in button-shaped encrustations, or in drops or studs, occasionally covered over with delicate rock crystals. They are of a beautiful bluish-white, and take a prominent place in any cabinet. On these are occasionally found small crystals of purple fluor-spar, from the size of a mustard seed to that of a small pea. Carbonate of lime appears as calcareous spar, frequently filling veins and cavities of a slightly-crystallized veined variety of marble, of various tints of brown, exactly like the Gozo marble seen at Malta, and the portions of the rock of Gibraltar from which ornaments are mostly cut. Sulphate of lime is found in veins in the form of beautiful fibrous gypsum, semi-transparent, and colourless. It also occurs in plates. Specimens of all the minerals here described I have in abundance in my own cabinet. They have been mostly collected for me by Dr Malcolmson, Mr Moyes, and Mr Adie; the duration of my own visits to Aden precluding me from procuring the rare minerals.

Right across the bay to the westward, at a distance of five miles, are the magnificent remains of another crater, called Gibbel Hasson. It is nearly of the same size and form as Aden, but rests on the mainland. The centre peak attains an altitude of 1237 feet, and on sailing round it from Aden, it, in certain aspects, presents the appearance of a stupendous Gothic cathedral. Two peaks of 700 feet, close beside each other, have obtained the very unpicturesque name of "The Asses' Ears," from the appearance presented by them far out at sea; while 7 miles beyond this, and 17

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from Aden, another fragment of a cone of smaller size, but considerable beauty, rises up to the altitude of 700 feet, and projects about 3 miles into the sea; while half-way betwixt this and the strait, Gibbel Kurruz, or St Anthony, apparently a volcano, reaches an elevation of 2772 feet; Barren Peak and the high range of Gibbel Arrar, or the Chimney Peaks, just opposite the strait, being all set down by the surveyors as hills of volcanic origin.* The range bends northward, and follows the line of the Red Sea shore.

From Aden to Bab-el-mandeb, indeed, the rocks along the Arabian shore seem to be wholly volcanic for a distance of above 100 miles. On the African shore a singular cove at the upper end of the Bay of Tadjoura, called Joobul Khareb, seems the crater of an old volcano. It is connected with the bay by two narrow channels, the whole width across, from coast to coast, being about three quarters of a mile, with a small island near the middle. One of the channels is 40 yards wide, with 16 fathoms water; the other 350, with 3 fathoms. The cove inside is about 13 miles in diameter, by six. The western portion is volcanic. At its extremity is a basin, or crater, 300 yards in diameter, surrounded by precipitous volcanic cliffs; though the sea makes its way to the water inside, the entrance is dry at low water. Lava and scoriæ abound everywhere around.† The waters of the cove are said occasionally to be violently agitated and disturbed without apparent cause, probably by the emission of gas from below,‡ the volcano being scarcely yet asleep. Off the outer bay the hills are of limestone, and rise to the height of 2000 feet.

The rocks around the salt lake Assal, whose waters are now nearly dried up and encrusted with salt, and are 590

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* The whole of this information is taken from the Charts and Survey Notices of 1836–40. Aden is the only volcano here I have myself examined, the others I have merely seen from sea. They have all the appearance of being correctly described, and, considering the ability of the surveyors, I have no doubt that they are so.

† Captain Burke's paper. London Roy. Geog. Trans., 1848.

‡ Harris' Highlands of Ethiopia, vol. i., p. 17.

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feet beneath those of the sea, are all volcanic on the eastern side. A bed of lava, containing several deep fissures, separate the waters of the lake from those at Gubat-el-Kherab, of which it appears to have been a continuation.* The lake is 11° 38′ 12″ N., and 42° 30′ 6″ E. It is about 7 miles across in its larger diameter, and 570 feet below the level of the sea. For about 300 miles westward into the interior the whole country seems volcanic. To the south-westward of this, near Shoa, is the volcano of Gibbel Abida, about 4000 feet high, its crater opening to the NW., and about 2½ miles in diameter, and further on the still higher peak of Aiullo. Here there is an even plain about 30 miles in diameter, studded with small cones, of which as many as twenty may be counted at once, each exhibiting a distinct and well-formed crater. The lava everywhere around is fresh and glossy, but no tradition exists of any eruption having occurred within the memory of man.

Returning to the Straits of Bab el-mandeb, we find the volcanic peaks of the High Brothers. On the far-extended African shore, the island of Perim, which occupies a portion of the straits near the Arabian side, with the Bab-el-mandeb Peak on the mainland close by, are masses of lava. Along the African shore, from lat. 11° to lat. 14°, and from long. 42° to long. 44°, the series of volcanoes is uninterrupted for the space of 400 miles, running into the interior about 10° N. towards Ankobar, long. 40° E.† How far the volcanic district extends into the interior along the African shore, within the Straits of Bab-el-mandeb, does not appear. A range of hills above 14 miles from the sea, to which it is nearly parallel, is set down on the chart as mostly volcanic. There is a second chain of very high mountains parallel to this again, about 50 miles further to the west, but its character does not appear to have been ascertained. On the Arabian shore, from lat. 13° to lat. 15° 40′, for a distance of nearly

* Dr Kirk's Journey from Tadjoura to Ankobar, 1841. Royal Geographical Trans, 1841, vol. x. The paragraph is given *verbatim*. *Ibid.*, and more extended, Bombay Geographical Transactions, vol. iii., 1841–44.

† Dr Kirk, *ut sup.*

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200 miles, a range of hills of volcanic origin is set down on the map about 20 miles from the shore, with a second range behind them, undescribed, like that on the African side. The lower range is a continuation of the Aden volcanoes, thus extending in a continuous line for above 300 miles alongshore. There can be no reasonable doubt that the whole basin of the Red Sea,—here about 100 miles across from the Arabian to the African chain of peaks,—is volcanic, studded, as the intermediate channel is, with cones, now in a state of activity; so that the ascertained area of this region, from Aden to near Ankobar, from this to Gibbel Teir, is 350 from E. to W., and 450 from S. to N. Within the channel of the Red Sea, the most conspicuous peaks are the Haruish Islands, and Gibbel Toogur, betwixt lat. 13° 40′ and 14°; the Zeybeyar Islands in lat. 15°; and Gibbel Teir in lat. 15° 30′. A violent eruption of short continuance occurred in the Zeybeyar Islands on the 6th of August 1846. Gibbel Teir has for nearly a century been known to be in a state of constant activity. It was visited by Bruce in 1774; it then gave out smoke, and was said occasionally to emit flame and stones; the masses of lava he describes as having shells imbedded in them, a circumstance that has not, so far as I have observed, been noticed by any other traveller.* It was visited by Captain Elman, when engaged in survey in 1838,† and by Dr Kirk in 1841. The island is circular, about 7½ miles round, resembling, on being approached, a hill of considerable elevation, rising from a plain, terminating in a bluff steep on the eastern extremity. The summit of the hill is about 300 feet above the sea-level,—there are no soundings close in-shore, at 150 fathoms, so that the visible portion is merely the summit of a hill, at present 1100 feet high, or 1900, if the altitude of 900 assigned it by the chart be correct, the base of which is hid by the waters.‡ The whole surface is covered with ashes, lava, and cinders; near the summit, there are about fifteen small open craters, from several of which steam and hot air are continually issuing, and occasionally smoke.

* See Travels, quoted in Geog. Society's Report for 1850.

† I have taken Dr Kirk's description, shortly abridged.

‡ Dr Kirk makes it 300, Bruce 500; in the Survey Chart it is set down at 900.

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Streams of indurated lava are seen to proceed from these chiefly towards the east side of the island. It is said to have been on fire about 1828 or 1830. One of the peaks in which it terminates, exhibits the remains of two craters of about 25 feet in diameter,—both have fallen in. A single crater of much more recent formation than the others, appears in the northern peak of Gibbel Teir, seems the northernmost of the volcanoes in the Red Sea, and probably limits the band.

We have no information whatever, so far as I know, as to any volcanoes in the interior of Arabia, or to the northward. In his description of the sulphur mines of Cummer (Khamir) opposite Kishmi, in the Persian Gulf, Lieutenant Jenkins* does not state whether the country is volcanic or gypseous, or both; and Lieutenant Fullarton, who presented specimens of crystallized sulphur† to the Bombay Asiatic Society in 1851, leaves us equally in the dark. We know that gypsum abounds in it. An extinct volcano, called Mount Nimrod, is described by M. Chaucourtois,‡ as existing near the Salt Lake Vau, in Armenistan, on the frontiers of Persia, between the 38th and 39th degrees of latitude. On the banks of the Euphrates, near the city of Hit, in the region of the Petroleum wells, Dr Winchester found scoriæ on the summit of a detached hill, about 80 feet above the level of the plain; but no other volcanic appearance was observed.§

Geology, as illustrated by Chemistry and Physics. By Professor GUSTAV BISCHOF of Bonn.‖ This Communcated by the Author.

Our earth moves in a space which must be at least as cold as the polar zones during the winter season. If at the

* Notice of the Sulphur Mines of Cummer, by Lieutenant Jenkins, Bombay Geographical Transactions, vol. i., 1836.

† Report of the B. B. R. As. Society, September 1851.

‡ Report of the Academy of Sciences, 17th November 1845. *Ibid.*, Edinburgh New Philosophical Journal, April 1846, p. 377.

§ Bombay Geographical Trans., vol. ii., p. 30.

‖ Translated, under the superintendence of Professor Bischof, from the Allgemeine Monatschrift für Wissenschaft & Literatur, Feb. 1852, by Dr Paul.

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creation it had the same low temperature as cosmical space, then it would have been gradually heated from the exterior inwards, by the influence of the sun, and would now present either an equal temperature throughout, or a decrease from the surface towards the centre. But precisely the contrary is observed; for at every part of the earth where, either in mining operations or by boring, any considerable depth has been reached, an increase of temperature presents itself. This is a fact which, since the time when Trebra, Saussure, and d'Aubuisson first directed attention to it, has been fully established by numerous observations. The earth, then, had, at the time of its formation, a higher temperature than cosmical space, and gradually lost a part of its heat to a certain depth below the surface. But in its interior it still preserves its original temperature, either not at all or in a very slight degree lessened; for in a body of such a magnitude as the earth, and consisting as it does of bad conductors of heat, the process of cooling is one of extreme slowness.

Some years since I took great pains to shew,* that warm springs occur in very great numbers in all rock formations, in all latitudes, at all heights above the level of the sea, as well as beneath it, if we consider as warm springs all such whose water is but one degree, or even less than that, above the mean local temperature. It is necessary that the significance of the term "warm springs" should be thus extended, for the mean temperature of cold springs is a function of the mean temperature of the air at the place of their occurrence. Every excess of heat, however minute, cannot therefore originate from this temperature, but must be owing to some other cause. A phenomenon so general as the occurrence of thermal springs can only be the consequence of a general cause. Since, now, the springs are warmer in proportion as they rise from a greater depth, since the water which fills and generally overflows the borings is warmer, the deeper the boring is extended, it is clear that the cause of this temperature exceeding the local temperature can

* See Physical, Chemical, and Geological Researches on the Internal Heat of the Globe. London, 1841.

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only be sought in the increasing heat of the earth towards the centre. The temperature of springs ranges from a few degrees above 32° Fahr. to the boiling point; consequently at a certain distance below the surface there must be a temperature of 212° Fahr.

So long as the few springs, which have a very high temperature, such as those of Carlsbad, Aix-la-Chapelle, Wiesbaden, &c., alone attracted the attention of physicists, it was not difficult to consider them as consequences of local conditions, as for example the burning beds of coal or iron pyrites. But since it has been shewn that an excess of temperature of only one degree demands explanation fully as much as the boiling temperature of the Iceland springs, it has become evident that it is only in rare cases that the phenomenon of hot springs depends upon local causes.

Volcanoes are phenomena which indicate the existence of a temperature in the interior of the earth very much higher than that of the hottest springs.

Davy's important discovery of the metals of the alkalies and alkaline earths led him to put forward a not very probable explanation of the high temperature which is possessed by the lava streams and ignited stones thrown out during an eruption. As these metals, even at ordinary temperatures, decompose water with considerable evolution of heat, it was possible, assuming the existence of these substances in the metallic state at certain depths, and the penetration of water, to conceive volcanic phenomena to be consequences of this decomposition and the heat thus developed. But admitting such to be the case, the hydrogen liberated would naturally issue from the craters in a strongly-heated state, and, coming in contact with atmospheric oxygen, burn there, giving rise to lofty flames, which would form a part of all volcanic phenomena.

While several observers will not admit the existence of such flames, still others have sometimes recognised them. However, if the oxygen of the alkalies and alkaline earths which constitute such a considerable part of lavas were derived from the decomposition of water, the evolution of hydrogen should be a very common phenomenon, and the quan-

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tity enormous. Moreover, the flames which have actually been observed appear to have been those of burning sulphuretted hydrogen, whose formation presupposes the existence not of metal, but of metallic sulphurets.*

Besides this, the assumption of the presence of these metals in the interior of the earth has very little probability. They could not in any case be situated in the limestone strata, which for example the crater of Vesuvius has broken through, nor indeed in any strata which have been formed by deposition from the sea, but must be supposed to exist in the primitive rocks which lie beneath all the sedimentary strata. It is not my intention here to speak of other difficulties which this hypothesis encounters.† The celebrated originator of it himself subsequently expressed a decided opinion that the increase of temperature towards the interior of the earth furnished a much more simple explanation of volcanic phenomena.

Wherever chemical processes attended by a development of heat takes place, the products of the action are found. Thus, in combustion smoke and ashes present themselves. If lava had been melted by such a process, then long before its flowing from the crater smoke must have issued as from our furnace chimneys. Where coal strata burn as at Duttweiler, near Saarbrücken, either melting masses of rock or heating them to redness, smoke ascends from fissures or through the planes of stratification. Nothing of this kind is seen to issue from the craters of volcanoes before the lava is poured out, for the vapours which ascend from them are of a totally different nature. It cannot, therefore, be subterranean processes of combustion which cause the melting of lava. Now, since the only other conceivable chemical process—a combustion of the metals of the alkalies and alkaline earths—is in the highest degree improbable, then, according to the present state of chemical science, there remains no other such process by which volcanic phenomena can be produced.

* Naumann Lehrbuch, d. Geognosie, Bd. i., p. 123.

† See Physical, Chemical, and Geological Researches on the Internal Heat of the Globe. London, 1841, p. 297, *et seq.*

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We are then compelled to return to the assumption of the pre-existence of melted matter in the interior of the earth, for it is this assumption alone which is supported by fact—the increase of temperature towards the earth's centre—all other hypotheses are destitute of such a basis.

The celebrated undertaking of the French Government in sending out the expeditions of Bouguer, Condamine Jussieu, &c., to Peru (1735) of Maupertius, Clairaut, Camus, Lemmonier, &c., to the polar regions of Lapland (1736), for the purpose of measuring degrees of latitude, and the subsequent measurements of degrees of longitude and pendulum observations, have demonstrated the flattening of the earth at the poles. By this means, the earlier views of Newton and Huygens, based upon the law of universal gravitation, and the centrifugal force of the rotating earth, as well as upon the assumption of a previous fluid state of the earth, were fully confirmed. As a consequence of these investigations, the question arose as to whether this former fluid condition of the earth was one of igneous or aqueous liquidity, whether, therefore, the earth at the time of its creation, was a melted or pasty mass. The state of igneous liquidity presupposes that a greater part of the water of the ocean, rivers, &c., if not the entire mass of water, was diffused throughout the atmosphere in the form of vapour; for, upon a melted surface, it could only have retained its liquid form under a most enormous pressure. This pressure can only be ascribed to the then existing atmosphere, constituted chiefly of aqueous vapour, and the gradual condensation of this vapour as far as the quantity which still exists in the atmosphere to the gradual cooling of the earth's crust; if, on the contrary, the earth was a pasty mass, then the water which had penetrated the solid materials must have gradually ascended from the interior to the surface; for in rocks, and especially crystalline rocks, water is either not found at all, or only in extremely small quantities, and as far as observation extends, this water has always been derived from the surface. If, therefore, the different rocks had originated from a pasty mass, the water must have separated by evaporation, somewhat in the same manner as it now separates from moist clay. The increase of tempera-

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ture towards the centre of the earth, would facilitate the explanation of drying of the earth's crust by this means.

Although we cannot altogether contradict the assumption that the earth was in a pasty condition at the time of its formation, still, the existing state of its interior, so far as we are acquainted with it, but little favours such a view. It is difficult to imagine the drying of the earth from the centre to the surface. On the other hand, the opinion that the interior of the earth is still in a pasty state, is opposed by the fact of the high temperatures indicated by hot springs and volcanic phenomena, as well as by the great density of the earth (5·44 or even 5·67), which leads us to infer that its interior must consist of much denser substances than its exterior crust; for any considerable quantity of water in the interior would lessen the density.

If, therefore, the flattening of the earth at the poles is assumed to be a consequence of its former liquid condition, then it is especially the condition of igneous liquidity which is the most in conformity with what we know of its interior.

But is it possible to entertain any rational doubt of the former liquidity of our earth and the other planets? If, by a calculation based upon an hypothesis, results are obtained which approximate closely with those obtained empirically, then this hypothesis acquires greater probability in proportion as these results correspond more accurately. Setting out from the assumption that the earth was formerly liquid, Newton obtained for the ellipticity of the earth a value which was somewhat greater than that which the subsequent measurement of degrees afforded. Huygens obtained a much smaller number. But these deviations cannot appear strange, since the theory presupposed many conditions which in reality could not have existed. The calculation acquired greater certainty through the theoretical investigations of Maclaurin, Clairaut, Legendre, and Laplace. The latter found the ellipticity $\frac{1}{330}$. Finally, Ivory subjected the problem to a thorough investigation, as much as possible free from limiting assumptions, and estimated the flattening of the originally liquid terrestrial spheroid at $\frac{1}{230}$, a result which corresponds as exactly with that obtained by Bessel from the ten most

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trustworthy measurements of degrees, $\frac{1}{289}$, as can possibly be expected from the difficulty of the calculation. The hypothesis of the former liquid state of the earth acquired by this means the highest degree of probability.

Liquid substances, in consequence of the extreme mobility of their particles, obey with the greatest readiness the action of forces which tend to alter their external configuration. Among these forces, gravity is that which, in comparison with all others, is so preponderating as almost entirely to obscure their action. It is only by observing very small liquid masses, in which the relative action of gravity is much enfeebled, that the influence of other forces upon the configuration of these masses can be recognised. Thus small drops of water and melted metals aggregate into spheres upon solid surfaces when the mutual attraction of their particles is greater than that between them and the surfaces upon which they rest. If it be desired to study larger masses of liquid bodies in their proper freely-assumed figure, we must consider the earth and the other planets in the forms which they have acquired through the united action of attractive and centrifugal forces while in their original liquid state. Theory points out that these masses must have assumed the forms of more or less oblate spheroids, and observation teaches us that such is really the case.

Plateau, professor of natural history in Ghent,* conceived the ingenious idea of placing large masses of fluids in circumstances which would neutralise the action of gravity without their ceasing to be subject to the other forces which tend to alter their exterior form. He effected this in a very simple manner, by introducing a fatty oil, such as olive-oil, into a mixture of water and alcohol, having the same specific gravity as the oil. The influence of gravity on this oil was entirely neutralised, for, as both liquids had equal densities, the oil only occupied the place of an equal mass of the surrounding liquid. Since fatty oils do not mix with a mixture of alcohol and water, the former remained floating in the surrounding liquid, and had perfect freedom to assume what-

* Poggendorf's Annalen, Erganzungs, band ii., p. 249, *et seq.*

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ever exterior configuration the forces acting upon it might tend to produce. This was confirmed by experiment; for the mass of oil, however large it might be, always assumed a perfect spherical figure while floating in the alcoholic liquid, and, as it were, imitated one of the planetary bodies suspended in cosmical space. By means of a simple centrifugal machine, Plateau succeeded in causing such a sphere of oil to rotate, and found that it became flattened at the poles, and spread out at the equator. When a considerable velocity was given to the sphere, it rapidly assumed the maximum of flattening, then it became hollow round the axis both above and below, and continued to extend more and more in a horizontal direction. Finally, it changed into a perfectly regular ring, and resembled that of Saturn. By modifying the experiment, he even succeeded in making a sphere of oil remain in the interior of the ring. Still Plateau recognised nothing more in this experiment than a scientific curiosity; for the circumstances which determined the result had evidently no similarity with those which have contributed to the formation of Saturn's rings.

Thus both theory and experiment lead to the inference that the earth and other planets have been formed from liquid masses. If there were a common solvent for all the substances existing upon the earth, it might be conceived that they had existed in such a solution at the creation. But such an assumption would be in contradiction to chemical laws. And as the assumption of an aqueous pasty condition is likewise an hypothesis which has but little foundation, there remains only the condition of igneous liquidity which is probable.

In favour of this, there are also analogies in the meteoric stones. If these, as Chladni conjectures, are older formations, which have long existed in some form or other in cosmical space, or if they have originated from pre-existing materials only a short time previously to their falling, the view that they are of cosmical origin is the most probable of all those which have been contrived for their explanation. According to all observations, they reach our earth in a more or less heated state—sometimes even red-hot. Many pre-

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sent impressions from which it may be inferred that they have been in a soft state. This view is also supported by the fact that some fire-balls appeared spherical only at first, and afterwards assumed a lengthened form.

I believe that I have sufficiently proved * that the heating of meteorites cannot, as Chladni assumed, be caused by the air, compressed in consequence of the immense velocity with which they move in space. Their heat cannot, therefore, be derived from without, but must stand in some kind of casual connection with their formation. The high temperature which, from the occurrence of hot springs, volcanic phenomena, and the polar flattenings, we infer our earth to have possessed at the creation, and still to retain in its interior, we now observe in the meteorites, which are probably fragments of exploded cosmical bodies.† A liquid state of cosmical bodies, caused by high temperatures, has then analogies in the meteorites, which, moreover, correspond in their composition with the formations upon our earth.

The lava which was thrown out during historical periods, and even before our eyes flows from volcanic craters, is very similar to that which, in prehistorical ages, flowed from extinct or still active volcanoes. The lava from old streams of Vesuvius, Ætna, and other volcanoes, the scoriæ, rapilli, and the volcanic ashes of these still active volcanoes, are not to be distinguished from the products of the extinct volcanoes in the Eifel and the neighbourhood of the lake of Laach. Upon Mosenberg, near Manderscheid, upon Falkenley, near Bertrich, &c., the transition of scoriæ into lava, and into actual basalt, can be traced. Nothing was therefore easier than to regard that basalt also which is found in other places, and far from either active or extinct volcanoes, as a lava-like production. The old lavas of Iceland, and the island Ischia near Naples, so perfectly resemble the trachytes which rise above the surface in steep cones, as in the Siebengebirge, near Bonn, that there was in this case also no reason to doubt a similarity of origin.

* Populäre Briefe an eine gebildete Dame über die gesamten Gebiete der Naturwissenschaften. Ertes Bandchen, 1848, p. 96, *et seq.*

† Ibid. p. 102, *et seq.*, and p. 126, *et seq.*

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However, notwithstanding the perfect mineralogical and chemical similarity, phenomena sometimes present themselves in basalt, which warn us not unconditionally to infer from this resemblance an entirely similar formation. Berger and Maculloch describe basaltic dikes of a few inches, and even less, in thickness, occurring upon the islands of Anglesey and Barra, and intersecting the rock in all directions.

Let us imagine a mass of matter in igneous fusion, which, after solidifying, yields a basaltic rock ascending in a fissure by volcanic agency, it must not be forgotten that this solidification would take place the sooner the narrower the fissure was. The flowing or ascent of masses in igneous fusion would therefore cease the sooner, the narrower the fissure; for the narrower it is the greater is the cooling action of the cold surfaces of the adjoining rocks. The width of a fissure, and the temperature of the ascending melted mass, therefore, determine the possibility of a penetration. If the fissure be too narrow, and the temperature of the mass too low, the penetration is no longer possible.* But there is yet another circumstance to be taken into consideration. The more rapid the solidification, the less is it possible for a melted mass to assume a crystalline form. If it takes place too rapidly, and this would be the case in proportion to the narrowness of the fissure, then only scoriaceous masses are formed. Therefore, even if the penetration can be conceived possible, still, if the circumstances are of such a nature that a slow solidification cannot take place, and the mass filling the fissures also appears as a true basalt, then it is necessary to be very cautious in assuming the crystalline basalt to have been formed by igneous agency. It must be left to the English geologists to ascertain, by an accurate revision of the narrow fissures and veins filled with basalt, how far the geological relations existing there can be reconciled with the laws of the crystalline solidification of melted masses.

Before attempting to include the basalt of all localities among the products of melted masses, endeavour should have been made to remove these and other difficulties. But

* Lehrb. der Chemischen u. Physik. Geologie, vol. ii., p. 740, *et seq.*

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these were lightly passed over. An unrestrained speculation, wandering far beyond the limits of strict scientific investigation, went still further. What was supposed to have been proved basalt of *one* species of a large class of crystalline rocks was extended to all crystalline rocks.

In the case of basalt, the analogy which it has with lava, and the transition of the one into the other, still remains a leading fact. But this analogy is wanting in other crystalline rocks; for nowhere has a transition of lava, whether it comes from extinct or still active volcanoes, into granite been met with.

Before taking such a bold venture as to declare granite, syenite, and similar crystalline rocks, to be products of a very gradual solidification of melted masses, the question should have been asked, whether the order of successive formation of the several mineralogical members of these rocks represents their solidification.* If crystals of different kinds, and different fusibility, were formed from a melted mass, it is evident that the least fusible would be formed first, and the most fusible last. Therefore quartz, the least fusible member of granite, would have crystallised first; feldspar and mica, being more fusible, would have crystallised last. The latter two would naturally have occupied the space left by the quartz. But, with the exception of the graphic granite, in which quartz and feldspar appear as simultaneous formations, precisely the contrary is found to be the case; the quartz occupies the space left by the crystals of feldspar. Consequently the more fusible feldspar solidified before the less fusible quartz. Fournet endeavours to remove this contradiction, by the assumption of a so-called condition of superfusion, according to which the less fusible quartz is supposed to remain soft at temperatures far below its melting point. But nothing is gained by this, more than the support of one hypothesis by another. The one falls with the other.

But it is not for this reason alone that the hypothesis of the igneous origin of granite falls to the ground. Other and no less important evidence is opposed to it.

* Lehrb. der Chemischen u. Physik. Geologie, Bd. II., p. 923.

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Quartz has never yet been found in a lava, or in any volcanic product immediately after the solidification, as a separate and not accidentally enclosed constituent. Such a fact could only have been overlooked by a volcanic zeal running wide of all physical and chemical laws. It was not until long after the hypothesis of the igneous origin of granite had been regarded as established, that some of the more moderate Plutonists were found to direct attention to this circumstance, and to admit that it could not be brought into harmony with the plutonic origin of granite, without, however, renouncing the general hypothesis.

The Plutonists found abundant occupation in explaining the formation of gneiss, a rock which, like granite, consists of quartz, feldspar, and mica, but differs essentially from it in possessing an evident stratification. The constituents of gneiss, then, speak in favour of a pyrogenous formation; according to the views of the Plutonists, its stratification indicates that it is a deposit from the sea. This contradiction separated the Plutonists into two different parties. The one consisted of those who did not question the sedimentary origin of gneiss but assumed a subsequent metamorphosis into crystalline rock to have been effected by igneous agency. The cause of this igneous metamorphosis they could not find in the sedimentary rocks themselves, but were compelled to seek for them in the adjoining rocks. This led to the hypothesis of plutonic metamorphism, according to which an amorphous sedimentary rock is supposed to be heated by contact with a melted mass ascending from the interior of the earth, and then to acquire a crystalline texture during the gradual cooling.

The other party consisted of those geologists who considered that the stratification and parallel structure of gneiss, and other stratified crystalline rocks, furnished no reasons for regarding them as other than pyrogenous formations, since their phenomena are equally marked in some lavas.* These views are opposed to all the evidence which exists against

* Naumann, Geognosie, p. 741.

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the formation of rocks by igneous agency, which has been partially mentioned already, and will partially follow.

The doctrine of plutonic metamorphism certainly found a feeble support in the transformation of glass into the so-called Reaumur porcelain. It was imagined that an uncrystalline sedimentary rock might, by ignition and gradual cooling, pass into a mixture of different crystalline minerals, in the same way that glass packed in sand is transformed by a red heat and gradual cooling into a crystalline mass.

It was also attempted to explain the metamorphism of rocks by the upward progression of the higher temperature in the interior. If, for example, a deep marine basin is gradually filled by sediment, the temperature of the original bottom would be raised. That such is really the case, is shewn by the increase of temperature in the sedimentary rocks formed in this manner. If the temperature increases at very great depths in the same proportions as in depths which can be observed, then at a depth of 115,000 feet in such a rock there would be a temperature of 1000° R. Such a heat may indeed exceed that by which glass is converted into Reaumur porcelain. At such a depth there would be no difficulty in conceiving a transformation of an amorphous sedimentary rock into a crystalline, if such a change actually does take place at a red heat.

Finally, it was imagined rocks might be so metamorphosed by the vapour and gaseous exhalations rising from craters, and in their neighbourhood, that crystalline rocks might be formed from amorphous strata.

There is no want of opportunities for the Plutonists to study the changes which rocks undergo when exposed to the continuous action of high temperatures. There are coal strata which have been burning for centuries. Argillaceous sandstone, clay, and slate clay, which form the roof of these burning strata, have, during this long period, been exposed to a very high temperature, and have been more or less altered. But they shew no other alteration than that observed in bricks after burning; they appear fritted, half vitrified, and scoriaceous. There has never been a crystalline mineral met with in them, such as feldspar, which occurs in all crys-

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talline rocks which are supposed to have been formed by plutonic metamorphism. But if, after the lapse of centuries, there does not appear to be any tendency to a separation of feldspar, then there is but little hope that they would be formed even after thousands of years.*

Another opportunity of judging the action of heat is afforded by the fragments of sedimentary rocks imbedded in scoriaceous masses. In the Eifel, the neighbourhood of the lake of Laach, at the Rodderberg, near Bonn, &c., such phenomena occur very frequently ; for instance, fragments of slate and graywacké imbedded in scoriæ. But here also, we recognise nothing more than a similarity to burnt bricks. The lake fragments frequently appear brick red, blistered at the edges, sometimes like pumice-stone, and in some cases vitrified.

The blistered character of such fragments shews that they had been half melted ; from the large size of some of these, we may infer a very gradual cooling ; Fakenberg, near Bertrich, 160 feet high, containing innumerable fragments of slate-clay imbedded in it, may, according to an approximative calculation, have required twenty-two years for its cooling. More favourable circumstances for a plutonic metamorphosis —a partial conversion of clay-slate into feldspar, the elements of which it contains—could not be found, were such a change possible, and still there has not even been a microscopic crystal of feldspar found in these fragments.† Every unprejudiced person, who is not blinded by attachment to an hypothesis, will doubt the possibility of such a transformation, and must do so until such a discovery shall be made.

For these reasons, we are not justified in assuming such a metamorphosis in sedimentary rocks, at depths where perhaps the central heat of the earth may have produced a very high temperature, such as these scoriaceous masses possessed in their former melted state ; since, as regards the action of heat, it is indifferent whether they are of one or the other origin. The thickest sedimentary formations, clay-slate and graywacké, have undoubtedly a very considerable thick-

* Lehrbuch der Chem. u. Physik. Geologie, Bd. II., p. 354.

† Ibid., p. 733.

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ness. It is possible that they reach the depth of 115,000 feet, and perhaps exceed it, for these strata have not been penetrated at any part of the earth. But if it were possible that clay-slate could at this depth be converted into a crystalline rock by the central heat of the earth, then a rocky mass must be removed to nearly such a depth, in order that the metamorphic rocks should be accessible. The frequent deep erosion of river valleys shews that considerable masses of dry land may be removed and carried into the sea by running water. If masses have been deposited at the bottom of the ocean to the thickness of several miles, then it is evident that equal quantities of debris must have been conveyed to the sea by this means. From this point of view, the hypothesis of a metamorphosis of the central heat of the earth would encounter less difficulties, if it were at all possible to regard such an hypothesis as a correct one.

With regard to the favourite hypothesis, according to which aqueous vapours are supposed to be the agents which cause the transformation of sedimentary into crystalline rocks, abundant opportunities are presented in nature, as well as in chemical laboratories and metallurgical furnaces, for acquiring a knowledge of their action upon rocks. During a space of 190 years, aqueous vapour has issued from fissures in clay-slate above the burning coal strata near Duttwiler, in such quantities, that in cold moist weather, the mountain valley is enveloped in dense clouds. Atmospheric air mixed with aqueous vapour and sulphurous acid at a temperature of 80° to 158° R., issues from some fissures. But besides a red colouring of the slate-clay throughout its mass, making it resemble burnt brick, besides sublimation of crystallised sulphur and chloride of ammonium, no other changes are recognisable. At other places, where plastic clay lies very near the focus of the combustion, this has been converted into porcelain jasper. The suffioni of Tuscany cause no other alteration in the adjoining jasper and hornstone than destroying their red and dark grey colour, and making the rock friable. At Terceira, aqueous vapours decompose the trachyte to white clay, silica is extracted and again deposited as hyalite.* Accord-

* Lehrbuch der Chem. Physikal. Geologie, Bd. II., pp. 353 and 354.

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ing to Jeffrey's experiments, aqueous vapour decomposes feldspathic rocks at the melting point of cast iron, and coats the roof of the furnace with a porous covering of silica. Aqueous vapours, therefore, act only as decomposing agents, and in no case produce crystalline formations.

Gaseous exhalations from the interior of the earth,—carbonic acid the most frequent, sulphuretted hydrogen the less frequent, sulphurous acid and hydrochloric acid, which are rare and occur only during volcanic eruptions,—exercise also a decomposing influence.

Although no trace of feldspar crystals has yet been found in the slate fragments imbedded in scoriaceous masses, still they occur in sedimentary formations; for instance, at many places in the Swiss Alps, far distant from all crystalline feldspathic rocks. Thus, according to Studer,* white quartz rocks, which pass into gneiss, lie in Churwalden in Bündten upon grey slate beds, several thousand feet in thickness, whose organic remains distinctly prove their sedimentary formation. Upon this quartz follows, as the uppermost covering, grey slate again. Here, therefore, the metamorphic gneiss is separated from crystalline rocks, as well as from the high temperature of the earth's interior, by thick strata of unaltered rocks. Neither those crystalline rocks, if supposed to have ascended eruptively, *i. e.*, in a melted state, nor the central heat of the earth, could, in this and many similar cases, have effected the metamorphosis, even if such a change were possible.

The wandering fancy of those geologists who maintained, and still maintain that the metamorphosis of sedimentary rocks was effected by their contact with crystalline rocks, went so far that they affirmed such an action to take place even at distances of 6000 feet, as if heated rocks acted like miasmas.† These same geologists, in many other cases, freely admit that the action of eruptive rocks upon the contiguous strata, either cannot be at all recognised, or only in a very slight degree. Studer, who, among other great merits, has

* Lehrbuch der Physikal. Geographie und Geologie. Vol. ii., p. 137.

† Lehrbuch der Chem. u. Physikal. Geologie. Bd. II., p. 353.

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that which is indeed not the least of having, more than any other geologist directed attention to the difficulties involved in the explanation of geological phenomena, by the hypothesis of plutonic metamorphism,* has observed, that at Mettenberg, near Grindelwald, limestone strata of scarcely more than an inch in thickness, surrounded on both sides by gneiss, neither lose their gray colour, nor their uncrystalline sedimentary character, and that distinctly preserved fossil remains (Belemnites and Ammonites), were situated almost at the surfaces of contact.

Should not this single fact have been sufficient to have shewn that this gneiss could not possibly have been formed by plutonic metamorphism, without the thin strata of limestone being at the same time altered, their colour and the fossil remains destroyed? Nevertheless, the more such facts are accumulated, the more do the plutonic notions of metamorphosis reappear again in a new form.

Such extravagancies in empirical sciences are but little adapted to inspire respect for the investigation of nature in the public mind. Would it not be more creditable for scientific men at once to admit their inability to explain complicated phenomena, than to contrive hypotheses which stand in contradiction to all sound reasoning? What kind of idea would be formed of the geologists of the nineteenth century, should these pages be read a century hence, and it should be seen that views such as that of plutonic metamorphism were maintained, and cost so much trouble to disprove?

That metamorphic processes go on uninterruptedly, as well in the inorganic as the organic kingdom, is an indisputable fact. If these cannot be explained by the action of heat, other methods must be adopted. I must now leave it to a future occasion to point out how far it has been possible to discover such.

* *Loc. cit.*, pp. 135, 158.

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*On the Physical Geography, Geology, and Commercial Resources of Lake Superior.** By J. J. BIGSBY, M.D., late British Secretary to the Canadian Boundary Commission, &c. Communicated by the Author.†

I. *Physical Geography.*

Lake Superior is included between W. longitude 84° 18′ and 92° 19′—and N. latitude 46° 29′—49° 1′. It is to the east of, and near to, the swell of high land which, stretching from the Rocky Mountains to Lake Superior, in wide undulating plains, divides the waters of the Mexican Gulf from those of Hudson's Bay;—and then, bifurcating, one fork proceeds on the north side of Lake Superior eastward towards Labrador, in groups of broken hills, while the other fork passes south-east as a rough and high country into the lowlands of the United States. It therefore occupies an oblong crescent-shaped hollow, with a general direction rather to the north of east. It has literally thousands of lakes on its north, and hundreds on its immediate south. It is 1750 miles round, 420 miles long, and 163 in extreme breadth. It is 597 feet above the Atlantic. Its greatest known depth is 792 feet. Soundings of 300, 400, 600 feet are common; but extensive shallows and flats prevail in parts.

The hydrographic basin of Lake Superior is singularly small, particularly on the south shore, where the tributaries of the River Mississippi and Lake Michigan often approach within 5 and 10 miles of the lake. It seems to be its own fountainhead.

The water is clear, greenish, extremely pure, pleasant to the taste, and soft from the nearly total absence of limestone from these regions. An imperial pint only contains $\frac{3}{10}\frac{1}{0}\frac{1}{0}$th part of a grain of mineral matters—carbonates of lime and magnesia, sulphate of lime, peroxide of iron, and the oxide of manganese.

The average annual temperature of the water is 40° F.; being about the same as that of the ocean at certain great depths. In June, the lake is often covered with ice; and in the middle of July, the surface-water freezes in the morning—with patches of snow in the clefts of the rocks. At this period of the year, or a few days later, the smaller lakes on the north are steadily at 72° and 74° F.

Lake Superior is not undergoing secular drainage. It is lowest in April, and highest by a few feet, in September. The great annual variations of rain of these countries produce corresponding changes of level. There are no tides, and no cycle of years for lake-levels.

Barometric changes produce curious local oscillations of level.

* The statements in this communication are partly derived from the able reports and charts of Messrs Bayfield, Logan, Foster, Owen, and others in the service of the Governments of Great Britain and the United States, Dr Bigsby's own researches on the northern shores of the Lake, for 440 miles, having supplied the remainder.

† Read in the Royal Institution.

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Thus the furious rapids, called the Falls of St Mary, on the river of discharge so named, are sometimes left dry. Messrs Foster and Whitney have seen the oscillation come from the centre of the lake in a wave 20 feet high—curling over like an immense surge, crested with foam, and breaking on the shore, diminishing as it approached it. On this occasion (Aug. 1845) it was the harbinger of a violent storm.*

The amount of water leaving the lake is small; for its outlet is often shallow, and the current weak.

The *Climate* is more arctic than temperate, although the lake is but little to the north of Milan. It is much colder than Sikla in Russian America, 10° further north; because the latter is screened from polar winds. Winter begins in the middle of October by a succession of gales and snow storms; and from November till May the ground is covered with close packed, granular snow; but the earth is not frozen deep, so that, in spring, before all the snow is gone, the forest is in leaf. The annual range of the thermometer is 125° F. the mean 42° 14′ F.; the lower extreme—31°, the higher 94°; all these observations having been made by good observers, with excellent instruments. August is the hottest month.

On a mean of 12 years, the winds blow about equally from all quarters; from the NW. the most frequently—from the south the least frequently.

The scenery of Lake Superior is striking;—its features are large and open (of which an example was shewn in a Sketch on the East Coast). The eye ranges over high lands and shoreless waters. The scanty and dwarfed woods of the north coast, the rocks, isles, and rivers full of cascades, have an impress of their own—not warm, soft, and umbrageous, like those of Lake Erie; but rugged, bare, and chill—arctic. The scene is oceanic,—the waves are large and high. Some of the plants, the *Lathyrus maritimus* and the *Polygonum maritimum*, for instance, on the beaches, and many of the insects disporting about, are those of the distant Atlantic.

In winter, Lake Superior might be called the "Dead Sea;" every living thing is gone, save the shivering inhabitants of some few white settlements. The Indian and the wild animals have retreated to the warm woods far away; and the sun looks down, from a bright blue sky, on the leaden waters, now narrowed by huge fields of ice—a small dark speck on an almost illimitable expanse of snow.

On the south shore, there are in the extreme east, high terraces and treeless plains of blown sand for many miles inland and along shore, succeeded by the high sandstone precipices, called the Pictured Rocks, battered into fanciful shapes by the violence of the waves. Then comes a low rocky coast for 200 miles or more, backed by

* A violent gale of wind, concurring with a local rise of level, will sometimes throw large stones or logs of wood 150 to 200 yards inland, and 30 to 40 feet above the usual water margin—as in three instances seen by Prof. Agassiz (L. Superior, pp. 95 and 106), and by Dr Bigsby. (Jour. Roy. Inst. xviii. 15.)

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dense forests, often mountainous, as at the Huron, Bohemian, and Porcupine Mountains. The scene is dark with the verdure of northern evergreens, and is here and there diversified with small clearings, and the smoke of distant mines ascending among the uplands. The bays are often deep, full of little iron-stained streams; and the promontories stretch for miles into the lake.

The eastern and northern shores are different—more naked, steeper, ever abounding in dome-shaped hills, or in ridges, rising by steps, scantily covered with trees either stunted or scorched with fire. (Large sketches were exhibited representing the lofty basaltic country about Fort-William, and the softer hill-scenery of Black Bay.)

With the exception of the Fur trading stations, there are no white settlements on the north shore; and this from its general barrenness. At the Peak River, soil was imported in bags with which to raise a few potatoes.

The *Fauna* and *Flora* of Lake Superior are semi-arctic, or subalpine. Professor Agassiz has treated of both in his late valuable publication on this lake. He found twenty-three new species of fish, and states that Lake Superior constitutes a special ichthyological district. The reason of this evidently lies in the coldness and extreme purity of the water, its slow departure towards the ocean, and the absence of weedy bays, and of lime rocks.

It would seem that some portion of its animal life are waifs and strays from grand geological periods long passed away—as we see in its herrings, minnows, and the new genus Percopsis. Connected with this subject, Prof. Agassiz conjectures that much of North America was dry land when the rest of the world was under water; and that thus its physical condition was less altered than elsewhere. Dr Bigsby was inclined to believe this; for had Canada been as long under water as other large tracts, we should probably have had in some part of its vast extent, a member or two, at least, of the mesozoic rocks; but there is no such thing—not a single relic of lias, oolite, or chalk, in the extraordinary heaps of debris which overspread these countries.

II. *Geology.*

The rocks of Lake Superior have been arranged under three principal heads, as follows :—

1. The *Metamorphic.*—Greenstone, chloritic, talcose, clay, and greenstone slates, gneiss, quartzite, jasper, rock and saccharoid limestone.
2. The *Aqueous.*—Calciferous sandstone, Cambrian sandstone and conglomerates.
3. The *Igneous.*—Granite, Syenite, Trap, in various states.

The place and extent of these rocks having been pointed out on a map, Dr Bigsby stated that the geological system of Lake Superior

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is a consistent and closely connected whole, forming a beautiful and easily read example of geological action in moulding the surface of our globe.

The lake may best be presented at once to the mind as a trough or basin of Cambrian (or Silurian) sandstone, surrounded and framed, as it were, by two orders of rocks, in the form of irregular and imperfect zones; the inner consisting of trap, with its conglomerates; and the outer, of metamorphic, flanking igneous rocks.

1. The *Metamorphic* rocks, with the exception of quartzite and jasper, are the oldest in the lake, and support great sheets of the above-mentioned sandstone unconformably; all these rocks being upheaved and altered by the intrusion of igneous rocks in instances innumerable. This group of rocks is entirely destitute of the traces of animal life.

The country they occupy on the south shore, with a general NNW. dip, may be best described as a rough table land of the various slates, out of which short hills of granite, gneiss, trap, &c., emerge in great numbers, with an almost constant east and west direction.

On the east and north shores the metamorphic rocks have a W. and WSW. strike, when visible. The slates of the north side of Michipicoton Bay run WNW., NW., and N.

The jasper and quartzite are merely altered sandstone and therefore younger than the other rocks of this group.

2. The *Aqueous* Rocks.—The youngest of these is calciferous sandstone. It exists as a broad band on the south-east shore, resting on the sandstone soon to be noticed. It is highly magnesian and siliceous in parts. A patch of it in Grand Island contains shells. (Logan.)

The Cambrian Sandstone seems to be the floor or basement of nearly all the lake, for the following reasons:—

1. Wherever it occurs, whether in immense sheets on the east and south shore, or in smaller areas on the north coast, it invariably dips towards the centre of the lake.
2. It can be recognised, paving the lake for some miles from the main in many places.
3. The soundings of Captain Bayfield exhibit, for large spaces, the uniformity of level to be expected from the presence of horizontal strata.
4. Because it constitutes Caribou Island, 40 miles from the nearest main land.

This sandstone is very ancient; and is supposed by Mr Logan to be Cambrian on the north shore, and lower Silurian on the south—a supposition, the latter clause of which, though extremely probable, is not yet established.

It has no fossils; but its ripple marks, impressions of rain-drops, and sun-cracks, are plentiful and perfect.

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It is more commonly red, and is composed of the debris of granitoid rocks, in nearly horizontal strata, except near intrusive rocks, when it rises to a high angle, hardens, and even passes into true jasper, porphyry, gneiss, or quartzite. There is reason to think that this sandstone is interleaved with trap. (A Landscape was exhibited of the Sandstone Rocks, south shore.)

The conglomerate is of the same age with much of the sandstone; and is almost invariably placed between it and the trap.

The conglomerates of Keweenaw and Isle Royale, consist of rounded boulders of trap, with a few jaspers, cemented by red iron sand; but those of Meminee and Nipigon contain also granites, quartzites, and sandstones; thus indicating a difference of age.

3. *Igneous Rocks.*—Granite everywhere forms the nucleus of an anticlinal axis, in two parallel lines running E. and W. on the south-east side of the lake, flanked by metamorphic and sedimentary rocks. Both it and syenite are plentiful.

Trap Rocks.—The ancient lavas of the lake are in very large quantities, and are well displayed. They are the great depositories of copper. For convenience sake, they may be divided into three principal forms.

1st. The highly crystalline mountain masses,—sometimes anticlinal and syenitic.

2d. The bedded trap, at various angles of inclination.

3d. Dikes intersecting igneous and metamorphic rocks.

They are all portions of one long series of volcanic operations.

Trap creates the great headland of Keweenaw, with its lines of stair-like cliffs and hills. (It was shewn in a large diagram and described as typical of the trap of the whole lake.) The trap of Keweenaw is met with in three contiguous and parallel belts, going WSW., and separated by bands of conglomerate, sometimes very thin, often numerous, and prolonged sometimes for 40 or 50 miles. These three belts have been named the outer, northern, and southern; the last being highly crystalline, or syenitic, and abounding in chlorite. It is an anticlinal to the rocks on both sides. The other two belts are bedded traps, and with their interleaved conglomerates dip northerly. They all coalesce at Portage Lake, and after proceeding to Montreal River, 130 miles in the whole, soon after disappear under horizontal sandstone westwards.

The north belt is the most metalliferous; and contains the celebrated Cliff and other rich mines. In the Keweenaw district it is the cross vein which yields the native copper—either in sheets and blocks or mixed in with the usual crystallizations, such as datholite, prehnite, stilbite, quartz, &c.

On the Ontonagon River the metalliferous veins run with the strike. The copper is pure, and has interspersed through its substance scales of pure silver; but without chemical union.

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The copper is confined to the trap, as a universal rule.

The north shore of Lake Superior is eminently trappose; and especially about Fort-William, where a region at least 120 miles long consists of basalt, amygdaloid, porphyries, jasper, conglomerate, and sandstone in the same mutual relations as on the south shore.

The trap dikes, traversing granites and other crystalline rocks indifferently, are a singular feature on the north shore, and abound chiefly from Written Rocks to the bottom of Michipicoton Bay. By their dark and undeviating course through the gray, red, or green rocks of the rugged coast, they strike the eye of the most incurious—if only as ruined staircases, crossing bays and headlands and climbing hills for miles. Their size, number, and direction are irregular. They may be solitary, or twenty in company—sometimes all parallel and close together. They often run with the general trend of the coast.*

Mr Logan divides them into three varieties, according as they are homogeneous, syenitic or porphyritic.

Professor Agassiz distributes the dikes of the whole lake into six systems—each with its own mineral character and direction—its own epoch of upheaval; and each he announces to have been an important agent in giving shape and direction to the district in which it occurs. He truly says that the general outline of the lake is the combined effect of many minor geological events taking place at different periods. With some truth in it, this theory does not seem to take into sufficient account the pre-existing metamorphic and granitic rocks, and it overlooks the variety observed in the directions of the dikes in the same neighbourhood.

Dr B. stated that if he might be allowed to hazard an opinion, it would be, that this curious assemblage of dikes—abounding as much in the S. as on the N. coast—pervading all the crystalline rocks indiscriminately, had ascended independently from the unseen, distant mass of trap beneath. They appear in many ways peculiar, and have no visible connection with the traps he had been describing.

Before the emergence of either traps or granites, Lake Superior received its *great* outlines from the metamorphic rocks,—thrown into their present position by still earlier upward movements: for on the eastern half of both shores of the lake they strike E. and W. with little variation; while on the western half, these far extending rock-masses strike WSW. and SW.,—giving thus to the lake a general eastward direction, with a gentle curve to the north, as stated before. This done, Cambrian sandstone slowly took possession of the trough of the lake—just as we see a certain shell marl is doing now. The anticlinal granites, which appeared afterwards,

* *Vide* Quart. Journal of Roy. Inst. vol. xviii., p. 244. Bigsby on Lake Superior.

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only concurred in the same effects; shaping and elevating the adjacent lands.

In after-geological times important modifications arose in the form of the lake. Promontories were pushed out, and islands raised up by successive outbursts and overflows of trap from separate fissures of great length—those for example of Keweenaw, Thunder Mountain, and Isle Royale—all intercalcated with conglomerates, formed in agitated seas between eruptions;—at different and most probably *distant* times, judging from the fact that some of the conglomerates are altogether trappose, while others abound in granite and other boulders.

We thus obtain the *general* order of all these events, and little more; but the knowledge is worth having. From the position of the uplifted mural cliffs, we see that the upheaving impulse came from the south-east.

Drift.—The groovings and striæ are almost always northerly here. New proofs are daily accumulating to shew more decisively the northerly origin of the foreign drift of Lake Superior. One of these is the fact that the limestone boulders on the north shore are upper Silurian,* and derived from the large calcareous basins some hundreds of miles north of Lake Superior: from whence Dr B. had brought characteristic fossils. Another is found in the occurrence of boulders of iron ore, in heaps, on the north side of certain cliffs, but which are absent on the south side—the original site of the ore being to the north of the cliffs, and near Lake Superior.

A Sketch was exhibited of a Wisconsin prairie, dotted with northern blocks dropped from icebergs.—From Dr D. Owen.

III. *Commercial Resources.*

Agriculture will only be carried on in parts of the south shore. Large quantities of white fish and of furs are annually exported.

The chief staple of Lake Superior is native copper. For ages before the appearance of Europeans in America, this metal was supplied from hence to the Indian nations far and near. The tumuli of the Mississippi, &c., contain the identical copper of this lake. Traces of ancient mining in Keweenaw, Ontonagon, and Isle Royale, are abundant, in the form of deep pits (a ladder in one), rubbish, stone mauls, hammers, wedges, and chisels of hardened copper. In a native excavation, near the river Ontonagon, with trees five hundred years old growing over it, lately lay a mass of pure copper 81 tons in weight, partly fused and resting on skids of black oak.

Modern explorers have hitherto only found two centres of metallic riches on the south coast,—that of Keweenaw and of Ontonagon. In the first are the valuable mines of the Cliff, North American,

* Containing Pentamerus, Spirifer, Leptæna (alternata) atrypa, various corals, minute trilobites, orthoceræ, and some cytherinæ.

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North-Western, and other companies. In the Ontonagon centre are the Minnesota and fifteen other mines.

At the Cliff mine three large steam engines are employed (1852); with 250 men;—and at the North American mine, two engines, with 160 men. Most of the other mines, forty in number, are assisted by steam-power. Three thousand miners are in work altogether, and the general population is fast increasing. Native copper is the principal object. Silver is always present, and occasionally in masses of considerable size. According to authentic accounts, dated February 1852, many new mines have been opened lately; and all are worked more systematically than heretofore,—generally by contract.

There are now in the Cliff mine masses of pure copper within view estimated to weigh 700 tons in the whole; and on the lands of the Minnesota Company, one block weighing 250 tons. The copper shipped in 1851 was about 1600 tons, valued at £130,000. This copper is stated to be of great excellence in the manufacture of wire, ordnance, and ship-sheathing.

The large beds of specular and magnetic iron ore, on the south-east side of the lake, are as yet only worked on a small scale.

At this moment the business of mining has ceased on the Canadian side of the lake. There is little doubt, however, but that profitable deposits will, sooner or later, be discovered here.

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*On the Heating Effects of Electricity and Magnetism.**

By W. R. Grove, Esq., M.A., F.R.S.

In the early periods of philosophy, when any unusual phenomenon attracted the attention of thinking men, it was frequently referred to a preternatural or spiritual cause; thus, with regard to the subject about to be discussed, when the attraction of light substances by rubbed amber was first observed, Thales referred it to a soul or spiritual power possessed by the amber.

Passing to the period antecedent to the time of more strict inductive philosophy, viz., the period of the Alchemists, we find many natural phenomena referred to spiritual causes. Paracelsus taught that the Archæus or stomach demon presided over, caused, and regulated the functions of digestion, assimilation, &c.

Van Helmont, who may be considered in many respects the turning point between alchemy and true chemistry, adopted with some modification the Archæus of Paracelsus and many of the opinions of the Spiritualists, but shewed tendencies of a more correctly inductive character; the term 'Gas,' which he introduced, gives evidence of

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* Delivered in the Royal Institution, February 13, 1852.

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the thought involved in it by its derivation from 'Geist,' a ghost or spirit. By regarding it as intermediate between spirit and matter, by separating it from common air, and by distinguishing or classifying different sorts of gas, he paved the way for a more accurate chemical system.

Shortly after the time of Van Helmont, lived Torricelli, who, by his discovery of the weight of air, was mainly instrumental in changing the character of thought, and inducing philosophers to introduce, or at all events to develope, the notion of fluids as agents which effected the more mysterious phenomena of nature, such as light, heat, electricity, and magnetism.

Air being proved analogous in many of its characters to fluids as previously known, the idea of fluids or of an ether was carried on to other unknown agencies appearing to present effects remotely analogous to air or gases.

Sound was included by some in the same category with the other affections of matter, and as late as the close of the last century, a paper was written by Lamarck to prove that sound was propagated by the undulations of an ether. Sound is now admitted to be an undulation or motion of ordinary matter, and Mr Grove considered that what have been called the imponderables, or imponderable fluids, might be actions of a similar character, and might be viewed as motions of ordinary matter.

Heat was at an early period so viewed, and we find traces of this in the writings of Lord Bacon. Rumford and Davy gave the doctrine a greater development; and Mr Grove, in a communication made by him at an evening meeting of this Institution in 1847, shewed that what had hitherto been deemed stumblingblocks in the way of this theory of heat, viz., the phenomena presented by what have been called latent and specific heat, might be more simply explained by the dynamic theory.

In this evening's communication, he brought forward some experiments and considerations in favour of the extension of this view to electricity and magnetism, an extension which he had for many years advocated, and which was, in his opinion, supported by many analogies.

The ordinary attractions and repulsions of electrified bodies present no more difficulties when regarded as being produced by a change in the state or relations of the matter affected, than did the attraction of the earth by the sun, or of a leaden ball by the earth; the hypothesis of a fluid is not considered necessary for the latter, and need not be so for the former class of phenomena.

In the cases of heating or ignition of a conjunctive wire or conducting body through which what is called electricity is transmitted, we have many evidences that the matter itself is affected, and in some cases temporarily, in others, permanently changed; thus if a wire of lead is ignited to fusion by the voltaic battery, the fused lead being kept in a channel to prevent its dispersion, it gradually shortens,

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and the molecules seem impressed with a force acting transversely to the line of direction of the electricity ; at length the lead gathers up in nodules, which press on each other as do, to use a familiar illustration, a string of figs.

With magnetism we have many instances of the molecular change which a ferreous or magnetic substance undergoes when magnetised. If the particles are free to move, as for instance iron filings, they arrange themselves symmetrically. An objection may be made arising from the peculiar form of the iron filings, but Mr Grove in the year 1845, shewed that the supernatant liquid in which magnetic oxide had been formed, and which contains magnetic particles not mechanically but chemically divided, exhibits when magnetised a change in the arrangement of the molecules, as may be seen by its effect on transmitted light ;—a molecular change is also evidenced by the note or sound produced by magnetism, and by other effects.

Assuming that the molecules of iron change their position *inter se* upon magnetisation, then by repeated magnetisation in opposite directions, something analogous to friction might be produced ; and just as a piece of caoutchouc when elongated produces heat (as it was on this occasion experimentally shewn to do), so a bar of soft iron might be expected when subjected to rapid changes in its magnetic state, to exhibit thermic effects.

With the aid of the large magnet of the Institution and of a commutator for changing the direction of the electricity, a bar of soft iron was alternately magnetised in opposite directions ; and in a few minutes a thermometer placed in an aperture in the iron shewed a rise of temperature of 1° 5′ Fahrenheit ; the bar being separated from the magnet by flannel, and the magnet being at a notably lower temperature than the bar, this heat could in nowise be attributed to conduction.

The effect of electricity in the disruptive discharge as in the voltaic arc and the electric spark, would seem at first sight to offer greater difficulties of explanation on the dynamic theory. The brilliant phenomenal effects of the electric discharge, and the apparent absence of change in the matter affected by it, would at first lead the observer to believe that electricity was a specific entity.

With ordinary flame or the apparent effects of combustion however, the idea has, to a great extent been abandoned, that such visual effects are due to specific matter, and it is regarded by many as an intense motion of the particles of the burning body. So with electricity ; if in regard to the disruptive discharge it can be shewn that the matter of the terminals or of the intervening medium is changed, the necessity for the assumption of a fluid or ether ceases, and, to say the least, a possibility of viewing electricity as a motion or affection of ordinary matter is opened.

To make evident to the audience the relation of the electrical discharge to combustion and the fact that the terminals were themselves

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affected, the voltaic arc was taken, first between silver and then between iron terminals; in the first case, a brilliant green-coloured flame was produced, and in the second, a reddish scintillation or spur-fire effect, just as in the ordinary combustion of the metals.

So with the discharge of Franklinic electricity between the same two metals, a strip of silvered leather gave the bright green discharge, while a chain of iron gave the spur-fire effect.

The known transport of particles of the terminals from one pole to the other,—the different effects of different intervening media on induction, as shewn in Faraday's experiments,—the polar tension of such media, &c., were instances of the train of molecular changes consequent upon electrical action.

Hitherto the polarity of the gaseous medium existing between the metallic or conducting terminals of the electrical circuit was only known as a physical polarity, and not shewn to have an analogous chemical character with that existing in electrolytes anterior to electrolysis; but Mr Grove stated that in a recent communication to the Royal Society he had shewn that mixtures of gases having opposite electrical or chemical relations, such as oxygen and hydrogen, or compound gases such as carbonic oxide, were electro-chemically polarized, or had their electro-negative and electro-positive elements thrown in opposite directions: thus, if a silvered plate be made positive in such gases it is oxidised, if negative the dark spot of oxide is reduced; and an experiment was shewn in which such a plate was thus oxidised and the spot reduced in gaseous media.

Here, as in the other experiments, was an effect on the terminals, and an effect of polarization of the intermedium. In the experiments hitherto shewn, solid terminals were used; it became important to examine what would be the effect of liquid terminals, for instance water; the spark or disruptive discharge of Franklinic electricity was readily obtained from its surface, but hitherto no voltaic battery had been found to shew a discharge at any sensible distance from the surface of water.

Mr Gassiot had procured to be constructed 500 cells of the nitric acid battery, the combination discovered in 1839 by Mr Grove, and first shewn at this Institution in the year 1840. The cells of this battery were all well insulated by glass stems, and as regards intensity of action it was probably far the most powerful ever seen. Mr Gassiot had kindly lent this apparatus for the illustratiou of this evening's discourse, and by its aid Mr Grove was able to shew an experiment which he had first made when experimenting with Mr Gassiot some time ago, and which produced the effect he had long sought for, viz., a quantitative or voltaic discharge at a sensible distance from the surface of water. The experiment was made as follows:—a platinum plate forming the anode of the battery was immersed in a capsule of distilled water, the temperature of which was raised. A cathode or negative terminal of platinum wire was now

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made to touch for a moment the surface of the water and immediately withdrawn to a distance of about a quarter of an inch; the discharge took place, the extremity of the platinum wire was fused, and the molten platinum attached to the wire, but kept up by the peculiar repulsive effect of the discharge, was exhibited, as it were, suspended in mid-air, giving an intense light, throwing off scintillations in directions away from the water, and only detaching itself from the wire when agitated.

Here water in the vaporous state must be transferred, for the immersed electrode gave off gas, without doubt oxygen, and the molecular action on the negative fused platinum resembled, if it were not identical in character with, the currents observed on the surface of mercury when made negative in an electrolyte.

It may be objected to the theory proposed, that electrical effects are obtained in what is called a vacuum, where there is no intermedium to be polarized; but this objection, though not applicable to the projection of the terminals, could hardly be discussed until experimentalists had gone much further than at present in the production of a vacuum. The experiments of Davy and others had shewn that we are far off from obtaining any thing like a vacuum where delicate investigations are concerned.

The view of the ancient philosophers, that nature abhors a vacuum, which had been much cavilled at, and was supposed to be exploded by the discovery of Torricelli, Mr Grove thought had been unjustly censured: giving the expression some degree of metaphorical license, it afforded a fine evidence of the extent and accuracy of observation of those who were unacquainted with inductive philosophy as a system, but who necessarily pursued it in practice. Whether a vacuum was possible might be an open question; experimentally it was unknown.

Lastly, in answer to those who might ask, To what practical results do researches such as these lead? what accession of physical comfort or luxury do they bring? Mr Grove took occasion to offer his humble protest against opinions now perhaps too generally prevalent, that science was to be viewed only or mainly in its utilitarian or practical bearings. Even regarding it in this aspect, were it not for the devotion which the love of knowledge, which the yearning anxiety to penetrate into the mysteries of our being and of surrounding existences induced; the practical results of science would not have been attained; the band of martyrs to science, from Socrates to Galileo, would not have thought and suffered without a higher incentive than the acquisition of utilitarian results. Without disparaging these results, indeed regarding them as necessary consequences of any advance in scientific knowledge, be considered that the love of truth and knowledge for themselves was the great animating principle of those who rightly pursued science; that, based upon an enduring quality of our common nature, this feeling was rooted in far firmer foundations,

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that it led to greater and more self-sacrificing exertions, than any capable of being induced by the hopes of augmenting social acquisitions, and was an attribute and an evidence of the non-transient part of our being.

On the Recent Progress of Ethnology, being the Annual Discourse for 1852. *Read before the Ethnological Society, at the Annual Meeting, on* 14*th May* 1852. By RICHARD CULL, Esq., Honorary Secretary. Communicated by the Ethnological Society.

We may congratulate ourselves on the continued and increasing interest which the educated classes of society are taking in ethnological knowledge. It is now practically admitted that the science of Ethnology is worthy of being cultivated. It is true that its claims have been tardily admitted; but we must remember that, in commercial countries, a science which does not promise to be pecuniarily profitable, has not those attractions which insure the devoted attention of crowds of students.

The desire of the public for systematic knowledge of our science, is evinced by the steady demand for the standard works on Ethnology, by the publication of so many new ones, by the popularity of lectures on the subject, and by the frequent introduction of Ethnology as a topic of conversation in general society.

Considerable attention was drawn to our science during the last year by the appearance of so many foreigners in London, who came to visit the Great Exhibition. And to witness the many varieties of man, assembled from every region of the earth in the Crystal Palace, calmly studying the productions of each other, was not the least of the wonders of that fairy-like creation whose physical existence is now about to pass away from us.

The differentia, both physical and non-physical, in the varieties of man, are so patent to observation, as not only to attract attention, but to rivet it, and absorb it. Some

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students, indeed, are unable to advance beyond the study of these differences. They constitute, however, only the threshold of our temple. Resemblances must also be studied. This tendency of the mind to fix itself on the differences in the varieties of man may be called the student's bias of mind. We occasionally find this bias of mind remaining in after life, and manifesting itself in over-estimating the value of those differences in relation to the resemblances.

Ethnology is a science of yesterday. Daubenton's observations on the situation of the foramen ovale, and Campers' on the facial angle, were the result of researches to discover a physical index to the mental capacity. In 1790 Blumenbach published his Anatomical Description of Ten Skulls, in order to shew how certain varieties of man differ from each other in cranial form. In 1820 Blumenbach completed his work, having altogether described sixty-five skulls.

In 1791 Dr Gall published the first part of an extensive work, in which we see the spirit in which his researches into the moral and intellectual nature of man were conducted. The great and fundamental principle was the comparison of cerebral form with the manifestation of mental qualities. In 1796 Dr Gall began to lecture at Vienna; and in 1798 we find him complaining, in a letter to his friend, Baron Retzer, that he was called a craniologist. "The proper object of my researches is the brain. The cranium is only a faithful cast of the external surface, and is consequently but a minor part of the principal object." In this letter he speaks of national heads in relation to national character. And to Dr Gall and his disciples we are most indebted for collecting crania, casts of crania, and casts of heads of the several varieties of man.

Ethnologists have much yet to do in collecting crania from various countries, and still more to do in ascertaining the relationship of these crania to each other, both in time and space. The mere geography of certain forms of cranium is but the first step of a great inquiry. We seek to know if one form of cranium passes into another. If so, under what circumstances, both physical and non-physical, does a mutation of form take place? Our Society might, perhaps, with advan-

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tage, draw attention to those ethnological questions which require solution to enable us to advance to higher generalizations.

The literature of Ethnology like that of other sciences, may be conveniently considered under two general heads, viz., as that which—

1. Advances Ethnology ; and
2. Diffuses a knowledge of Ethnology.

It is quite true that a work written to advance the science, does also, to a certain extent, diffuse a knowledge of the science ; but a work that is written to teach the science does not necessarily advance it, and hence the distinction. During the past year, works have been written with both these objects in view. I proceed briefly to notice them.

Three works on Ethnology from the pen of one of our Fellows, Dr Latham, have appeared since last May. "The Ethnology of the British Colonies and Dependencies" is a small work, which represents a course of lectures which Dr Latham delivered at the Royal Institution, Manchester, during February and March 1851. "Man and his Migrations" is another small work, and which also represents a course of lectures which the author delivered at the Mechanics' Institution, Liverpool, in March 1851. These works being devoted to the teaching of what is known, *i.e.*, to the diffusion of Ethnological knowledge, require no further notice on this occasion.

Dr Latham's edition of the Germania of Tacitus, with Ethnological dissertations, is a valuable contribution to our science. The object of the work, is to exhibit in detail, the Ethnology of ancient Germany. The means of effecting this object is the study of the different languages of the families and nations descended from and allied to the Germans of Tacitus. We all know the difficulty of reconciling the Ethnology of Germany at different epochs in the historic period with the Germania of Tacitus. The question, as Dr Latham notices, is not whether certain nations of the Germaniæ are rightly placed therein, but whether Tacitus' test of Germanism was the same as ours ; and whether, if different, more correct.

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We know that nearly the whole of that part of the Germania of Tacitus east of the Elbe, as well as certain parts west of the Elbe, were, at the beginning of the proper historical period, *i.e.*, in the reign of Charlemagne not Germanic but Slavonic. Did Tacitus confound Slavonians with Germans? Did the German population of Tacitus entirely abandon that tract of country, and a Slavonian population take its place? These are questions which Dr Latham has treated in detail with great ability. The country east of the Elbe was only dimly sketched by Tacitus. The period when it became known in detail, and from personal knowledge, is the reign of Charlemagne. Accurate geographical knowledge of this region was scarcely possible to Tacitus.

It is not enough, as Dr Latham remarks, to know how a modern writer classifies the varieties of man. The reader of Tacitus must also know the view that the ancients took of those varieties. The ancients had clear notions of the differences between the group to which they themselves belonged, *i.e.*, the Classical group, and the groups to which the so-called βαρβαροι belonged. This notion, clear as it was, was limited to one direction. It comprehended only the points of difference. Modern science has extended the notion to comprehend points of resemblance also. The resemblances which brought the Slavonians and Goths into the same group with the Classical stock—the great group called Indo-European, were utterly unknown to the ancients.

"The Unity of the Human races proved to be the Doctrine of Scripture, Reason, and Science. By Thomas Smyth, D.D."

This work, which appeared in Edinburgh late last autumn, is a reprint, revised and much enlarged by the author, of the American edition. The American edition grew out of three discourses, and various articles by the same author, which appeared in several Theological Journals in the United States; and which were written to controvert the position of Professor Agassiz, that the origin of the human race is multiform. Professor Agassiz is well known as a profound naturalist, whose opinions are entitled to much respect. As a naturalist, the Professor declares there is no common or se-

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veral centres of origin for the lower animals, but that they were all created in the localities which they naturally occupy, and in which they breed, either in pairs or multitudes. The same is asserted of man. There is no common central origin for man, but an indefinite number of separate creations from which the races of man have sprung. The Professor fortifies this opinion by an appeal to the Holy Scriptures. He says the Biblical history of the creation of man is that of only one race, viz., that of Adam; and that Adam and Eve were not the only pair created, nor even the first created of human beings.

Professor Agassiz thinks there was a distinct origin and separate creation for each race. And that each creation took place in that locality which the race naturally occupies.

On these views Dr Smyth joins issue; and in some very able controversial writings and lectures, has strenuously argued for the unity of the human race. Those controversial works have formed the basis of a systematic work in which he labours to shew, that Scripture, reason, and science concur in proving the unity of the human race. Dr Smyth's eloquent and forcible book partakes in some degree of the controversial spirit, but even with that drawback, it is well worthy the attention of ethnologists.

Dr Smyth devotes three chapters to the statement of the Historical and Doctrinal Evidence of Scripture on the unity of the human race. "This doctrine, be it observed, Scripture teaches us, not as a matter of scientific knowledge, but as the foundation of all human obligation, and of the *universality* of all human charity."—(P. 112.) But as regards ethnological details, "the tenth and eleventh chapters of Genesis are unquestionably the best ethnographical document on the face of the earth."—(P. 100.) The importance of the doctrine to Christianity cannot be overrated; for, "it will be at once perceived that the gospel must stand or fall with the doctrine of the unity of the human races."—(P. 112.)

With so high an estimate of the importance of this doctrine to our highest interests, we are prepared for the urgency with which it is advocated. As a theologian, Dr Smyth places in the front rank his Scripture evidence. "The truth

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and certainty of the unity of the human races has now, we believe, been established as an incontrovertible fact. It rests upon the unmistakeable evidence of the infallible Word of God, who, in the beginning, made of one blood all the nations of men to dwell on all the face of the earth." But without bating a jot of the value of his theological argument, Dr Smyth boldly enters the ethnological arena, fully convinced that ethnological conclusions must be drawn from ethnological data; and, accordingly, he discusses the question "of the nature and philosophy of species," and then ably argues that, "The unity of the races is proved by the unity of the species;" that "the unity of races is proved by their common fertility, and by the infertility of hybrids," and then grappling with the philological argument that, "the unity of the races is proved from the universality, nature, and connection of languages."

Dr Smyth then quits the scientific data and reasoning, to call in the aid of history and tradition; and thence he appeals to experience, and the insensible gradations of the varieties, as arguments for the unity of the races of men. Dr Smyth does not evade the difficulties which surround his view of the question. Professor Agassiz and other mere naturalists, lay great stress upon the fixedness of the physical characters of the several varieties of man, that these characters have been fixed certainly from the earliest dawn of history; whence it is argued that the races of men have *always* been separated by the same amount of differences. Dr Smyth fairly states the argument of the Professor, and devotes a chapter to the "Origin of the Varieties of the Human Species," and this chapter is well worthy of attention. Amongst the interesting portions of the book is an appendix, "On the former Civilisation of Black Races of Men;" and a note "On the Veddahs of Ceylon."

In several respects Dr Smyth's is a remarkable and valuable work. It may be considered as a contribution to the bibliography of our science. It is a compendium of all that has been written on that side of the question. His authorities are duly cited, without any parade of learning, without egotism, without any assumptions of superior sanctity; and with strict impartiality does he state his opponents' views.

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There is with all this so much originality and deep interest in the subject, that the reader is carried along, without at all thinking of the author himself.

Our associate Mr Logan's Journal of the Indian Archipelago and Eastern Asia continues to record most important information of the Malay and other peoples of that Archipelago. The history, antiquities, languages, and ethnology of the Malays is gradually being brought to European knowledge by Mr Logan, and his band of contributors living at and around Singapore, who are so praiseworthily labouring for science in that distant region.

Mr Logan's extensive knowledge of Malay dialects gives a peculiar value to his philological researches, and his intimate knowledge of the physical, intellectual, and moral character of the Malays give him great vantage-ground in his ethnological researches of the Indo-Pacific islands. The ethnology of the central Malay nations is now becoming more distinct, and is assuming a more definite outline, which will clear the way to a fuller appreciation of the facts connected with the outlying tribes and offshoots which are so widely scattered over the islands of the Pacific. I commend to your especial attention Mr Logan's researches, which you will find recorded in the 4th and 5th volumes of his Journal of the Eastern Archipelago.

Captain Denham is now about to sail with an expedition which is placed under his command to make a survey of certain groups of islands on the east of Australia. We may therefore expect new and valuable details of the Ethnology of that region.

I think the time has now arrived when a report on the Ethnology of the Pacific Islands might be drawn up, by which means special attention would be drawn to the nature and amount of our ignorance of the details of that ethnology.

We all regretted the early death of Captain Owen Stanley, who died in command of the Rattlesnake, while on her surveying voyage. Mr Macgillivray, the naturalist to the expedition, has written an account of the scientific results of the voyage. Very important knowledge has been collected in various departments of science. Amongst others, some

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valuable contributions have been made to Ethnology. The survey of Torres' Straits was important in a commercial and maritime point of view. The Ethnology of this district, including both sides of the Straits, Timor, &c., is most important in relation to the great questions of the route of migration of the Malays to people the islands of the Pacific, and that of the black race (Papuans?) to people N. Australia. Materials are now fast collecting, which will, it is hoped, ere long enable us to form some connected view of the Ethnology of this region.

"Steene Bille's Bericht der Reise der Galathea um die Welt."

This book is an account of a voyage of discovery round the world of the Galathea, a Danish corvette, commanded by Captain Bille.

The great object of the expedition was to obtain more accurate and positive knowledge of the Nicobar Islands, a Danish colony in the Bay of Bengal, with a view to that Government's decision as to the expediency of retaining the colony. The Galathea proceeded by the Cape of Good Hope to Madras, Calcutta, and these islands. Thence she passed through the Straits of Malacca, to Batavia, the Philippines, China, the South Sea Islands, and home by Cape Horn.

The Danish edition is in three volumes, the German, by omission of an appendix and other curtailing, is compressed into two volumes. I have only seen the German edition. The voyage occupied two years, from 1845 to 1847, and the delay of publication was caused by the political events which have so much agitated Europe for the last four years. The chief value to us is the very complete account of the Ethnology of the Nicobar Islands. The appendix contains vocabularies of the Nicobar and Negrito languages, which I regret are not in the German edition, and the scientific (ethnological) part has evidently been abridged also. This I regret; for while German is generally read, we find but few who read Danish.

We are indebted to another of our fellows, Dr Daniell, for gathering important details of the Ethnology of Western Africa. Dr Daniell has resided for several years on the

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Guinea Coast, where he has laboured for the advancement of several sciences. His paper on the natives of Old Callebar, printed in our first volume, established his reputation as an ethnologist. His recent papers on Akkrah and Adampé will sustain that reputation. He has filled up lacunæ in our knowledge of African Ethnology; and we may confidently look for more knowledge as the result of new investigations which he will enter upon on his return, at the end of the month, to Africa. And let me add, that he will have our best wishes for the preservation of his health, and that he may be spared to return to us, and to increase his reputation by enlarging still more the boundaries of our ethnological knowledge of West Africa.

We may also expect additions to our African ethnology from Dr Overweg's expedition.

"A Manual of Geographical Science. By the Rev. C. G. Nicolay." The several articles of this manual are written by various authors. The article Physical Geography is written by Professor Ansted. The section on the distribution of animals in space and time occupies 48 pages; that on Ethnology occupies 25 pages; so that a very fair proportion of space is allotted to our science. But in so small a space there can only be a sketch of the subject. The sketch is a compilation, chiefly from the works of Dr Prichard and Colonel Hamilton Smith; and, as might be expected, the sketch is more a description of the geographical distribution of the human family than a manual of Ethnology.

Our science has been advanced, and most valuable materials have been collected for others to advance it, by the Bible and the several Missionary Societies. The philological and other knowledge which those societies have collected together to further the high and noble objects which they have in view, has also been of incalculable value in advancing ethnological science. I need not enumerate the many unwritten languages which the missionaries have been the first to study and reduce to writing. Nor is it necessary to enlarge upon the great patience and ability which is required so to acquire, and afterwards to write; for the works themselves speak louder than any praise of mine. The labours

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of the missionaries have greatly contributed to advance the bounds of our knowledge in both glossology and grammar.

Colonel Rawlinson has read another memoir to the Royal Asiatic Society, on the Babylonian and Assyrian Inscriptions, part of which, occupying about 150 pages, has recently been published in the Journal of that Society. The memoir is upon the Babylonian translation of the Great Behistun Inscription. The memoir consists of—1st, The text and an analysis of the Babylonian inscription at Behistun; 2d, An indiscriminate list of Babylonian and Assyrian characters; and, 3d, On the Babylonian alphabet, of which we have only the beginning now published.

It can be shewn, beyond all doubt, that a very large proportion of the Assyrian signs, *i. e.*, the arrow-headed characters are polyphones. "But although I can thus shew the probable reason of the employment of cuneatic polyphones—although I can explain the fact of the character 𒆳, the ideograph for a 'country,' being invested with such discrepant phonetic values as *mat* and *kur*, by referring to the Semitic synonyms מָת in Chaldee, and كورة in Arab. (cognate with χώρα),—the practical inconvenience of such a variableness of power is excessive. The meaning, for instance, of an Assyrian or Babylonian word may be ascertained determinately, either from the key of the trilingual inscriptions, or from its occurring in a great variety of passages with only one signification that is generally applicable; but unless its correspondent can be recognised in some Semitic tongue, it is often impossible, owing to the employment in it of a polyphone character, to fix its orthography. In the multitudinous inscriptions, again, of Nimroud, of Khursabad, of Koyunjik, and of Babylon, of which (although their general application can be detected without much difficulty) the details require for their elaboration a minute philological analysis, this orthographical uncertainty presses on the student with almost crushing severity. On the one side, in working out his readings, he can only employ philological aid,—that is, he can only compare Hebrew or Chaldee correspondents, after being assured of the true sound of the Assyrian and Babylonian word; while on the other, he must

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depend on his acquaintance with Semitic vocables to fix the fluctuating cuneiform powers."

The recovery of a long-lost language like the Babylonian is, of itself, a matter of deep interest, but the consequences of that recovery, in enabling us to read the numerous inscriptions containing ancient records of Babylonian history, and enabling us also to trace the philological relationships of that language, are consequences of great interest and value to us as ethnologists. It appears that the Babylonian is a Semitic language, and that Biblical Hebrew and Chaldee are the two languages greatly used by Col. Rawlinson to illustrate it. I take this opportunity to refer you to a remarkable passage in the Divinity Lectures of the Rev. W. Digby, Dean of Clonfert. I quote from a copy printed in Dublin in 1787. In Lecture V. the Dean is engaged in shewing that the confusion at Babel was not of language in its ordinary sense, but about religion.

"That the whole earth was of one religion, and *that* the true one, immediately after the flood; when but eight persons were left alive, is not to be disputed. That there was also but one language then spoken; and that a variety of languages did not immediately take place upon the confusion at Babel, but was the slow, gradual, and natural effect of the dispersion, is fairly to be collected from the book of Scripture. Nay, more, that but one language was spoken for several ages after, will, I think, appear from the following circumstances.

"It is allowed on all hands, that Canaan, the son of Ham, spoke the same language with Abraham, Isaac, and Jacob. It appears further, that Nimrod, who was grandson to Ham, spake the same language with Asher, the son of Shem: the former in Babylon, the latter at Nineveh."

The Dean carries on his argument to shew that one language only was spoken up to the time that Joseph was carried into Egypt. And he argues that this universal language was the Mosaic Hebrew. I have quoted the Dean not to follow his argument in detail, upon which, on the present occasion, I pass no opinion, but to shew that some ground exists why we, as modern philologists, might have

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been justified in assuming, *a priori*, that should the Babylonian language be recovered, it would be found to be cognate with the Mosaic Hebrew.

It was proposed some years ago to appoint a distinct Section for the cultivation of Ethnology in the British Association for the Advancement of Science. The proposal was negatived by the committee of the Association. It was, however, felt, that Ethnology is worthier than to occupy a mere subordinate place in the section of Natural History. It has accordingly been removed from that section, and now is united with Geography, which has been removed from Geology, and they together form one section. If by such a union Ethnology were to be degraded into the science of the geographical distribution of the human race, I should never cease to raise my voice against its union. At the Ipswich meeting last July, Section E, for the cultivation of Geography and Ethnology, met for the first time, and you were duly informed of the results of that meeting at the opening of the session.

I have now rapidly glanced at the progress of Ethnology during the past year. In relation to the whole science, that progress is fragmentary, but so is the annual progress of every other science. One of the chief uses of reviewing our progress is to draw attention to what is well known, less known, and unknown; for our knowledge and ignorance are so blended as to make a chaos. And, in reviewing our position, it would be well to draw up special reports in order to affix the boundaries of our knowledge, so as to exhibit in its full magnitude our ethnological ignorance, both in relation to space and time.

Common experience in the progress of knowledge shews, that in proportion as the number of students of a science increase, so does a larger number of persons become interested in the advancement of that science, and we find it advance. The history of Mathematics, of the Physical Sciences, of Chemistry, of Geology, of Geography, and other sciences, all concur in shewing that a rapid advancement of the science, both by extension of knowledge, and a fuller comprehension of the laws of nature, invariably follows a wider diffusion of what

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is already known. Such being a law of relation between the advancement and the diffusion of knowledge, I take the liberty of asking you to aid us in advancing Ethnology by diffusing as widely as possible a knowledge of it amongst your friends, and thus to awaken in them an interest in our pursuit. In this way, however small our knowledge may be, and however unable immediately to enter upon original researches ourselves, we may still indirectly conduce to the advancement of our science. And let us not hesitate to pursue our inquiries, lest they should land us in scepticism; for that which refuses to investigate in case of such a result, is a scepticism of the worst kind; for it is a scepticism whose assumption is, that the words of God are at variance with His works; and will, therefore, continually place Religion and Science in antagonism. Let us remember, in the beautiful language of Spurzheim, that "genuine philosophy and genuine religion are very nearly akin. The one explores the elder volume of nature—the other investigates the later volume of Divine revelation. Both unite in their practical results; both promote the present improvement of man; both conduce to his ultimate felicity."

Chemical Report to the Lords of the Committee of Privy Council for Trade, on the cause of Fire in the Ship Amazon. By Professor GRAHAM.

MY LORDS,—In reply to the questions arising out of the disastrous loss of the Amazon by fire, which are proposed to me for a chemical opinion, I beg to submit to your Lordships the following statements and conclusions.

The practice of mixing together the various stores of the engineer, consisting of oils, tallow, soft-soap, turpentine, cotton-waste, and tow, and placing them in heated store-rooms contiguous to the boilers, must be looked upon as dangerous in no ordinary degree, for several reasons. Although oil in bulk is not easily ignited, particularly when preserved in iron tanks, still, when spilt upon wood or imbibed by tow and cotton-waste, which expose much surface to air, the oil often oxidates and heats spontaneously, and is allowed to be one of the most frequent causes of accidental fires. The vegetable and drying oils used by painters, are most liable to spontaneous ignition, but no kind of animal or vegetable oil or grease appears to be exempted from

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it; and instances could be given of olive-oil igniting upon sawdust; of greasy rags from butter, heaped together, taking fire within a period of twenty-four hours; of the spontaneous combustion of tape-measures, which are covered with an oil varnish, when heaped together, and even of an oil-skin umbrella put aside in a damp state. The ignition of such materials has been often observed to be greatly favoured by a slight warmth, such as the heat of the sun. I am also informed by Mr Braidwood, that the great proportion of fires at railway stations have originated in the lamp-store, and that in coach-works also, when the fire can be traced, it is most frequently to the painter's department, the fire having arisen spontaneously from the ignition of oily matters. Lamp-black and ground charcoal are still more inflammable, when the smallest quantity of oil obtains access to them, and should not be admitted at all among ship's stores.

The stowing metallic cans or stoneware jars of either oil or turpentine in a warm place, is also attended with a danger which is less obvious, namely, the starting of the corks of the vessels, or the actual bursting of them by the great expansion of the liquid oil, which is caused by heat. These liquids expand in volume so much as one upon thirty, by a rise of not more than 60° of temperature, or by such a change as from the ordinary low temperature of 40° to a blood heat; the latter temperature may easily be exceeded in an engine-room. It is remarkable that the burning a few years ago of a large steamer on the American lakes, which even surpassed in its fatality the loss of the Amazon, was occasioned by the bursting, in the manner described, of a jar of turpentine placed upon deck too close to the funnel, by a party of journeymen painters who were passengers. This steamer was also on her first voyage, and being newly varnished, the flames spread over her bulwarks and extended the whole length of the vessel in a few minutes.

The bulk-heads of coal-holds appear to admit of obtaining considerable security from fire by being constructed double where close to the boiler, with a sheet of air between the two partitions. The tendency of coals to spontaneous ignition is increased by a moderate heat, such as that of the engine-room, from which they would be protected by the double partition. I have obtained instances where coals took fire in a factory, on two different occasions, by being heaped for a length of time against a heated wall, of which the temperature could be supported by the hand; also of coals igniting after some days upon stone flags covering a flue, of which the temperature was not known to rise above 150°, and of coals shewing indications of taking fire by being thrown in bulk over a steam-pipe. These were Lancashire coals, which are highly sulphureous; but the same accident occurred with Wallsend coals, at the Chartered East Company's Works in London, where the coals were twice ignited through a two-feet brick wall, of which the temperature was believed by Mr Croll not to exceed 120° or 140°.

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The surface of deal in the partition opposed to the boiler, would probably be better protected from fire by impregnating the wood with a saline solution, which diminishes combustibility, such as the zinc solution of Sir W. Burnett, rather than by coating the wood on the side next the boiler with sheet iron. Indeed, this use of iron appears to introduce a new danger. The iron being a good conductor of heat, the wood below is heated nearly as much as if uncovered, and wood in contact with iron appears to be brought by repeated heating to an extraordinary degree of combustibility, and to become peculiarly liable to spontaneous ignition.

Mr Braidwood, who has been led to that conclusion, gave an instance of wood covered by sheet-iron igniting spontaneously in a wadding manufactory. The numerous occasions, also, on which wood and paper have been ignited by Perkins' heated water-pipes, equally exemplifying the dangerous consequences which may arise from moderately heated iron, in long contact with combustible matter.

The most obvious precautions for guarding against the spontaneous ignition of coal stowed in ship's bunkers, appear to be the taking the coal on board in as dry a condition as possible, and the turning it over, if there be room for doing so, as soon as the first symptom of heating is perceived. An obnoxious vapour is described as always preceding the breaking out of the fire, and affords warning of the danger. The ignition of Newcastle coals in store, is not an unfrequent occurrence at the London gas-works. It appears always to begin at a single spot, and is met by cutting down upon and removing at once the heated coals. Long iron rods are placed upright in the coal heap, which can be pulled out, and indicate by their warmth the exact situation of the fire. Steam can be of little avail for extinguishing the fire among coals in bulk; and water, although it may extinguish the fire for the time, is too apt to induce a recurrence of the evil.

For extinguishing a fire occurring in berths or cabins in the immediate vicinity of the boiler and engine-room, steam might be more advantageously applied, means of turning on the steam being provided upon the upper deck, or other distant place of safety. Steam, however, can only be said to be efficient in extinguishing flame, or a blaze from light objects, and is not to be relied upon beyond an early stage of a fire. Upon a mass of red-hot cinders the extinguishing effect of steam is insensible.

An essential condition of applying steam with success to the extinction of a fire in the engine-room, would be to prevent the rapid ingress and circulation of air at the same time, which is occasioned by the draught of the fires. This could only be done completely by valving the chimneys; for the quantity of heated air passing off by the funnels, greatly exceeds in volume the steam produced by the boilers in the same time, and would rapidly convey away the steam

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thrown into the atmosphere of the engine-room, and prevent any possible advantage from it.

The fire in the "Amazon" appeared to the witnesses to take its rise either in the small oil store-room situated over the boiler, or in a narrow space of from three to eleven inches in width between a bulk-head and the side of the boiler, immediately under the same store-room. No substance remarkable for spontaneous ignition, such as oiled cotton-waste, was actually observed in the store-room or the space referred to. The wood itself of the bulk-head, which was within a few inches of the boiler, may have been highly dried and sensibly heated by its proximity to the latter, but is not likely to have acquired any tendency to spontaneous ignition; for when that property results from low heating, it is an effect of time, requiring weeks or months to develop it. The same observation applies to the decks in contact with the steam-chest, which encased the base of the funnel.

Nor does it appear probable, that the coals in the coal-hold of the vessel gave occasion to the fire by heating of themselves, and then burning through the wooden partition of the oil-store, with which they were in contact.

The coals were from Wales, and are not remarkable for this property. They are also said to have been shipped in a dry and dusty state, and not damp, a month or two previously.

Their ignition would also have been preceded by the strong odour before referred to, which does not appear to have been remarked, although the coal-hold communicated directly with the boiler-room.

Oil was seen to drop from the floor of the store-room upon the top of the boiler, but not in greater quantity than might be accidentally spilt in drawing the oil from the tank for the use of the engineers.

A parcel of twenty-five newly-tarred coal-sacks, which had been thrown upon the boiler, also obtained, it is supposed, some of the same oil. This oil appears to be the matter most liable to the possibility of spontaneous ignition, which was noticed near the spot where the fire commenced.

But the sudden and powerful burst of flame from the store-room, which occurred at the very outset of the conflagration, suggests strongly the intervention of a *volatile* combustible, such as turpentine, although the presence of a tin can of that substance in the store-room appears to be left uncertain. It was stated to be there by two witnesses, but its presence is denied by a third witness. I find, upon trial, that the vapour given off by oil of turpentine is sufficiently dense at a temperature somewhat below 110° to make air explosive upon the approach of a light. Any escape of turpentine from the heated store-room would therefore endanger a spread of flame, by the vapour communicating with the lamps burning at the time in the boiler-room, or even with the fire of the furnaces.

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The fire appears not to have begun in the tarred sacks lying upon the boiler; although, from their position, which was close to the store-room, they must have been very early involved in the conflagration, and contributed materially to its intensity. The sacks appear to have been charged each with about two pounds of tar, thus furnishing together fifty pounds of that substance, in a condition the most favourable that can be imagined for rapid combustion. The freshness of the tar, and its high temperature would make it ignite by the least spark of flame, although not prone to spontaneous ignition. The burning of a group of newly-tarred cottages in Deptford, which came under the notice of Mr Braidwood, arose from their being set on fire by lightning, while the sun was shining upon them, and the tar liquified by the heat.

The origin of the fire must remain, I believe, a subject of speculation and conjecture; but the extreme intensity, and fearfully rapid spread of the combustion, are circumstances of scarcely inferior interest, which are not involved in the same obscurity.

The timber of the bulkheads and decks near the engine-room is reported to have been of Dantzic red wood, or Riga pine, and such was the character of a portion of the Amazon's timber which was supplied to me for chemical examination. The wood has had its temperature drawn off, and differs in that respect from pitch pine. The Dantzic red wood is, in consequence, less combustible than pitch pine, but more porous and spongy. Oil paint is absorbed, and dries more quickly upon this porous wood than upon oak and other dense woods. After the paint is well dried, pine and other woods certainly acquire from it some protection from the action of feeble and transient flames, which might kindle the naked wood. But the effect of paint—especially of fresh paint—appears to be quite the reverse when the wood is exposed to a strong, although merely passing, burst of flame. The paint melts, and emits an oily vapour which nourishes the flame, and soon fixes it upon the wood. There can be no doubt, therefore, that the timber of the Amazon was in a more inflammable state than ship-timber usually is, from being recently painted, and also, probably, from its newness and comparative dryness.

But the circumstance which appears above all others to give a character to the fire in the Amazon was its occurrence, not in a close hold or cabin, but in a compartment of the vessel where a vigorous circulation of air is maintained by the action of the boiler-fires and their chimneys. The air of the engine-room must be renewed, under this influence, every few minutes, and would be so although full of flames rising above deck through the hatchways; for a portion of these flames would always escape by the funnels, and add to their aspirating power instead of diminishing it. The combustion of bulkheads or decks, once commenced in this situation, would therefore be fanned into activity, and powerfully supported.

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The destruction of the floor of the oil store-room, and the overturning, in consequence, of the oil-tanks and combustibles into the well of the boiler-room, was probably the crisis of the fire. A mass of combustible vapour would speedily be generated, and shot about on all sides, of which the kindling power upon the new and painted timber of the bulkheads and decks would be wholly irresistible.

The burning of the Amazon impresses most emphatically the dangerous and uncontrollable character of a fire arising in the engine or boiler-room, where the combustion is animated by a steady and powerful circulation of air, and the danger of collecting combustible matter together in such a place. The removal of the oil stores to a safer locality is, fortunately, generally practicable, and is the measure best calculated to prevent the recurrence of any similar catastrophe. I have the honour to remain, Sir, &c.

THOS. GRAHAM.

To the Lords of the Committee of
Privy Council for Trade.

—*The Quarterly Journal of the Chemical Society*, No. xvii., p. 34.

On the Foliation and Cleavage of Rocks of the North of Scotland. By DANIEL SHARPE, Esq., F.R.S., V.P.S.G.

Mr Sharpe, in a paper read to the Royal Society of London on the 15th January 1852, applies the term, *cleavage* or *lamination*, to the divisional planes by which *stratified* rocks are split into parallel sheets, independently of the stratification; *foliation*, to the division of *crystalline* rocks into layers of different mineral substances; *slate*, to stratified rocks intersected by cleavage; and *schist*, to foliated rocks only which exhibit no bedding, independent of the foliation.

He considers that no distinct line can be drawn between gneiss and mica schist, chlorite schist, &c., which pass from one into the other by insensible gradations; have the same geological relations, and foliation subject to the same laws. He states that their boundaries have been laid down arbitrarily on the published maps of Scotland. The quartz rock of MacCulloch includes two formations; the one a quartzose variety of gneiss, included in this paper under that head; the other, a stratified sandstone altered by plutonic action.

The author treats the foliation of gneiss and schist as a

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series of simple curves, obtained by observing the general direction, and disregarding the minor and more complicated folds. The convolutions are usually greatest where the dip is slightest, but where the foliation is vertical, or nearly so, it usually follows true planes without contortion; thus the most correct observations are those taken where the foliation is vertical.

When the foliation of gneiss and schist is traced over extensive areas, and the minor convolutions disregarded, it is usually found to form arches of great length, and many miles in diameter, bounded by vertical planes, between which the inclination increases with the distance from the axis. Each arch is succeeded by a narrow space, in which the dip is irregular, and beyond which another arch commences of a form similar to the first. Portions of two adjoining arches seen without the rest form the fan-like structure observed by several geologists. The arrangement of the foliation in arches corresponds with that of the cleavage of the true slates previously described by the author, except in the greater convolution of the gneiss and schist.

Along the southern border of the Highlands, a band of stratified clay-slate rests on mica schist; at the junction, the foliation of the schist conforms to the cleavage of the slate, and the two together form an arch, but there is no connection between the stratification of the slate and the foliation; moreover, the divisional planes cross from one rock to the other, without change of direction, being planes of foliation in the mica schist, and of cleavage in the slate; these facts confirm Mr Darwin's opinion, that cleavage and foliation are due to the same cause.

The author describes the parallel arches of foliation which cross the Highlands, illustrating his description by sections and a map, on which they are laid down, and tracing in detail the vertical planes which bound the arches. Commencing on the south, the first vertical plane runs about four miles within the Highland border, with a mean direction of about N. 55° E.: it crosses more than once the junction of the clay slate and mica schist. South of this plane the cleavage of the slate forms the beginning of an arch, which

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ends abruptly at the junction of the slate with the old red sandstone.

To the north of this vertical plane four arches run across the Highlands; the most southern of these, with a diameter of ten or twelve miles, is formed partly of the cleavage of the slate, and partly of the foliation of the mica schist. The hills on the south side of Loch Tay coincide with its central axis. The vertical plane which forms its northern boundary crosses Ben Lawers, and has a mean direction of N. 50° E. The next arch northward, consisting principally of gneiss, has a diameter varying from twenty-five to thirty miles; its axis runs for some distance along the central ridge of the Grampians. The granite of Cruachan and Ben-Muich-Dhui interferes with the regularity of the foliation of this district, and the lines are thrown to the north by the granite of Aberdeenshire; the line which bounds this arch on the north crosses the Spey near Laggan, and runs N. 40° E. through Corbine into the Monagh Leagh mountains. To the north of that line, the foliation of the gneiss forms an arch only ten miles wide, bounded on the north by a vertical plane running N. 35° E., which crosses Coryaraick. This plane forms the southern boundary of an arch, varying from fifteen to twenty-five miles wide, entirely of gneiss, bounded on the north by a band of vertical foliation which runs about N. 30° E., from Glen Finnan through the middle of Ross-shire, and across Ben Nevis. To the north-west of this band there is half an arch in the foliation, varying from twenty to thirty miles wide, which ends abruptly at a line to be drawn from Loch Eribol and Loch Maree, on the west of which the gneiss is unconformable to that hitherto described, but agrees with that of the island of the Lewis, forming a series of arches which run about NW.

From the want of parallelism in the lines of foliation of the Highlands, they would all nearly converge between Lough Foyle and Lough Swilly among the mica schists of the north of Ireland.

The most rugged and elevated hills are usually on or near the lines of vertical foliation; the axis of the arches are generally found in high land, and the principal valleys occur between the central axis of the arches and their vertical

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boundaries. Thus the main physical features of the Highlands are connected with the foliation of the gneiss and schists; but the granites and porphyries which have broken through those rocks, and disturbed the regularity of the foliation, have also greatly modified the surface of the country.

The contortions of gneiss and schists being unaccompanied by fracture, must, the author considers, have been produced when the matter of those rocks was semifluid; in this state the mineral ingredients appear to have separated and re-arranged themselves in layers according to their affinities, while the whole was subjected to pressure acting along certain axes of elevation, which raised those layers into arches.

On the Structure of the Iguanodon, and on the Fauna and Flora of the Wealden Formation. By G. A. MANTELL, Esq. LL.D., F.R.S.*

The geological phenomena of the south-east of England, comprising the lithological characters and organic remains of the Diluvial, Tertiary, Cretaceous, Wealden, and Oolitic deposits, were described in two Lectures delivered to the Members of the Royal Institution by Dr Mantell in 1836 and 1849. In those discourses the fauna and flora of the *Wealden* were cursorily noticed, and the Iguanodon and other gigantic terrestrial reptiles, whose fossil remains have invested the strata of Tilgate Forest with a high degree of interest, were briefly alluded to. The present lecture was restricted to a consideration of the fauna and flora of the countries whence the deposits constituting the Wealden districts were derived; and the osteological characters of the most remarkable fossil Saurians peculiar to this geological epoch were especially illustrated.

After a concise exposition of the characters of the various formations which have succeeded, and now overlie, or, in other words, are of more recent origin than the Wealden—namely, the *Drift*, or Diluvium, containing bones of large mammalia, as the mammoth, mastodon, rhinoceros, horse, deer, &c.;—the *Eocene*, or ancient tertiary strata of the London basin, abounding in marine exuviæ of special and for the most part extinct types;—and the *Cretaceous* or chalk-formation, comprising the white chalk of the North and South Downs, and the chalk-marl, galt, and greensand of Surrey, Kent, and Sussex, the whole characterised by innumerable marine shells, zoophytes, fishes, reptiles, &c. of extinct species and genera;—Dr

* Read in the Royal Institution on March 5, 1852.

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Mantell proceeded to illustrate the structure of the Iguanodon as exemplified by the isolated parts of the skeleton hitherto discovered, and of which numerous examples were exhibited on the tables of the Institution.

The perfect germ, and the unused tooth, of the Iguanodon, are characterised by the prismatic form of the crown, which is traversed on the thick enamelled face by three or four longitudinal ridges, and has the lateral margins denticulated, and the summit finely crenated; in this state the teeth resemble those of the living *Iguana* of the West Indies—a resemblance which suggested the generic name of *Iguanodon*. But the fossil teeth are of enormous size in comparison with their recent prototypes; for the teeth of the Iguana are as small as those of the mouse, while those of the Iguanodon are often one inch wide, and three inches in length. Specimens exhibiting the above characters are, however, rare; the summit of the crown is usually more or less worn away by use, and the fang removed by absorption from the pressure induced by the upward growth of the successional teeth. In the first example discovered by Dr Mantell (in 1820), the crown was ground down so as to present on its inner face a smooth oblique surface with a cutting edge on the summit, and the marginal crenations were worn away. In this state the fossil so strikingly resembled an upper tooth of a rhinoceros, that Baron Cuvier pronounced it to belong to a species of that genus. Numerous teeth in different stages of growth and detrition were at length obtained, and the reptilian character of the animal to which they belonged was satisfactorily determined. Three years since, the first specimen of the lower jaw was discovered by Captain Lambart Brickenden, in the same quarry in Tilgate Forest from which the earliest known tooth was obtained; and subsequently a portion of the upper jaw with teeth has been procured from the Hastings strata.

Referring to his various memoirs on the Iguanodon in the Philosophical Transactions, and to his recent work on the Organic Remains in the British Museum,* for details, the lecturer stated, that while the compound structure of the lower jaw, and the mode of dentition, established the reptilian character of the original animal, the maxillary organs presented a nearer approach to those of certain mammalia, than is observable in any other reptiles. The teeth in the upper and lower jaw were arranged in a sub-alternate order as in ruminants; the face of the crown, or that having the thickest coat of enamel, is placed mesially or on the inner side of the lower teeth, and on the external surface of the upper. The anterior part of the lower jaw is edentulous, and its symphisial extremity forms

* "Petrifactions and their Teachings, or a Hand-book to the Gallery of Organic Remains in the British Museum," one vol. 1851, published by H. G. Bohn.

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a scoop-like process, which resembles the corresponding part of the inferior jaw of the Edentate mammalia, as for example the *Mylodons:* and the great number and size of the vascular foramina of the jaw indicate a greater development of the lips and integuments than occurs in any existing animals of the class Reptilia; the sharp ridge bordering the deep groove of the symphysis, in which there are likewise several foramina for the exit of nerves and bloodvessels, evidently gave attachment to the muscles and integuments of the lip; while two deep pits for the insertion of the protractor muscles of the tongue, manifest the mobility and power of that organ. There are, therefore, strong reasons for supposing that the lips in the Iguanodon were flexible, and, in conjunction with the long, fleshy, prehensile tongue, were the chief instruments for seizing and cropping the leaves, branches, and fruit, which, from the construction of the teeth, we may infer constituted the food of the original. The mechanism of the maxillary organs, as elucidated by recent discoveries, is thus in perfect harmony with the remarkable characters which rendered the first known teeth so enigmatical; and in the Wealden herbivorous reptile we have a solution of the problem, how the integrity of the type of organisation peculiar to the class of cold-blooded vertebrata was maintained, and yet adapted, by simple modifications, to fulfil the conditions required by the economy of a gigantic terrestrial reptile, destined to obtain support from vegetable substances; in like manner as the extinct colossal herbivorous Edentata, which flourished in South America, countless ages after the country of the Iguanodon and its inhabitants had been swept from the face of the earth.

The structure of the cervical, dorsal, and caudal vertebræ, of the ribs, the pectoral and pelvic arches, the sacrum formed of six anchylosed vertebræ, the bones of the extremities, and certain dermal appendages, were successively described, and illustrated by drawings and specimens. From the facts adduced Dr Mantell infers that this stupendous reptile equalled in bulk the largest herbivorous mammalia, and was as massive in its proportions; for living exclusively on vegetables, the abdominal region must have been greatly developed. Its limbs were of proportionate size and strength, to support and move so enormous a carcass; its length, as proved by recent discoveries, was of crocodilian proportions, for there is no doubt that the tail was very long; and the largest Iguanodon may have attained a length of from fifty to sixty feet.

The *Hylæosaurus*, *Megalosaurus*, and several other genera of reptiles were severally noticed, and reference made to the specimens in the British Museum. The *Pelorosaurus* was next described somewhat in detail, and the characters of the stupendous humerus, or arm-bone, ($4\frac{1}{2}$ feet long), scapula, clavicle, vertebræ, sacrum, and pelvis, were pointed out, with a view of illustrating a most interest-

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ing discovery made but a few days previously by S. H. Beckles, Esq. of St Leonard's.

With much labour and skill, Mr Beckles had succeeded in extracting from a block of Wealden sandstone lying on the Sussex coast, and which was only visible at low water, the perfect *radius* and *ulna* (bones of the fore-arm), and *humerus* (arm-bone), of a gigantic reptile, which Dr Mantell pronounced to be a *new species of Pelorosaurus*, and proposed to name *Pelorosaurus Becklesii*. The generic identity and specific difference between this humerus and that of the *Pel. Conybeari*, which was placed beside it, were pointed out, and the remarkable modification of structure presented by the *ulna* was explained. The arm-bone of the *P. Conybeari* is 54 inches long, the corresponding bone of a Gavial or Gangetic Crocodile 18 feet long, in Dr Grant's Museum, is but 11½ inches; the humerus discovered by Mr Beckles is 22½ inches in length, and the bones of the fore-arm are 16 inches long. A portion of the scaly cuirass which covered the limbs, and is composed of hexagonal plates, was exhibited.

The lecturer then took a rapid view of the other reptiles that were contemporary with the Iguanodon, enumerating the *Pterodactyles* or flying lizards, and several genera of Crocodilians and Chelonians. Examples of marine and fresh-water turtles are not uncommon in the Wealden deposits; and the strata near Swanage have furnished many beautiful specimens to the researches of Mr Bowerbank.

Of *Fishes* there are nearly forty known species in the Wealden, which are chiefly referable to the *Ganoid* and *Placoid* orders. The fishes most abundant in the rivers of the Iguanodon country were two or three species of *Lepidotus*,—ganoids closely allied to the *Bony* or *Gar-Pike* of America; their teeth and scales are everywhere to be met with in the Tilgate strata.

The *Invertebrate Fauna* comprised many genera of *Insects*, a few *Crustaceans*, and numerous fresh-water *Mollusca*. The *Insects* (for a knowledge of which we are mainly indebted to the scientific acumen of the Rev. P. Brodie) amount to several hundred specimens, comprising between thirty and forty families or genera, and are referable for the most part to the orders *Coleoptera*, *Orthoptera*, *Neuroptera*, *Hemiptera*, and *Diptera*. Among them are several kinds of beetles, dragon-flies, crickets, May-flies, and other familiar forms which are closely allied to species that inhabit temperate climates.

Mollusca.—The most numerous shells belong to the genera *Cyclas* and *Paludina*; of the latter, which is a genus of fresh-water snails, there are a few species that abound in the Wealden clays and Purbeck beds, and form extensive strata of shelly limestone, the compact masses of which are susceptible of a good polish, and are

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well known by the names of Sussex, Petworth, and Purbeck Marble; the latter was in great request in the mediæval ages, and is the material of which numerous tombs and monuments, and cluster columns in our ancient cathedrals are constructed. Two common inhabitants of our pools and streams, the Planorbis and Limneus, also occur. Several species of *Unio*, some of which rival in magnitude the pearl-mussels of the Ohio and Mississippi, likewise abound in the Wealden deposits. Fresh-water Entomostraceans, *Cyprides*, of several species, swarm in many of the clays and ironstone beds of Sussex and the Isle of Wight.

The Flora of the country of the Iguanodon appears to have been as rich and diversified as the Fauna. Forests of *Coniferæ*, referable or closely allied to *Abies Pinus*, *Araucaria*, *Cupressus*, and *Juniperus*, clothed its hills and plains: with these were associated arborescent and herbaceous Ferns, comprising upwards of thirty species; together with many *Cycadeaceæ*, and trees allied to the *Dracæna*, *Yucca*, &c. Equisetaceous and Lycopodiaceous plants also abounded; and even the common inhabitants of our streams, the *Charæ*, flourished in the rivulets of that marvellous region.

As examples of the vegetation of the Wealden period, Dr Mantell described the petrified forest of Coniferæ and Cycadeæ in the Isle of Portland: the accumulation of fossil firs and pines exposed on the southern shore of the Isle of Wight; and the coal-field of Hanover, which entirely consists of the carbonized foliage, trunks, and branches, of coniferous trees, drifted from the country of the Iguanodon.

The facts thus rapidly noticed prove that during the deposition of the Wealden, Oolitic, and Cretaceous strata, there existed an extensive island or continent, diversified by hills and valleys, and traversed by streams and rivers teeming with fishes, crustaceans, and mollusca, closely allied to types which at present inhabit the fresh water of temperate regions; and that with these were associated fluviatile turtles, and crocodilian reptiles, whose living analogues are restricted to tropical climes. Colossal herbivorous and carnivorous saurians, differing essentially in structure from all known existing forms, were the principal inhabitants of the dry land; and these, together with flying lizards, and possibly a few birds, and very small mammalia, constituted the vertebrate fauna of the country, or countries, which supplied the materials of the Wealden strata, and of the fluvio-marine deposits which are intercalated with the purely oceanic beds of the oolite and chalk.

Thus it appears, according to the present state of our knowledge, that the classes Mammalia and Aves, which constitute the essential features of the terrestrial zoology of most countries, were represented through a period of incalculable duration solely by two genera of very diminutive mammals, and a few birds; while the air, the land, and the waters, swarmed with peculiar reptilian forms, fitted for aerial, terrestrial, and aquatic existence.

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Admitting to the fullest extent the effect or causes that may be supposed to have occasioned the absence of mammalian remains in the secondary deposits, yet the immense preponderance of the reptile tribes is unquestionable. Some authors have attempted to account for this anomaly by assuming that antecedently to the Eocene period, our planet was not adapted for the existence of mammalia, in consequence of its atmosphere being too impure to support higher types of animal organisation than the cold-blooded vertebrata. But the certainty that some forms of marsupial and placental mammalia inhabited the countries of the Megalosaurus and Pterodactyle, —that birds in all probability existed with the Iguanodon,—and the fact that insects and mollusca, and trees and plants, which now inhabit regions abounding in birds and mammalia, flourished during the "Age of Reptiles," demonstrate that the physical conditions of the earth, and the constitution of the atmosphere and of the waters, differed in no essential respect from those which now prevail, and that the laws which govern the organic and inorganic kingdoms of nature have undergone no change.

That the class Reptilia was developed during the periods embraced in this discourse, to an extent far beyond what has since taken place, appears to be indisputable; nor can any satisfactory solution of the problem be offered from the data hitherto obtained. Future discoveries may however shew that coeval with the country of the Iguanodon there were regions tenanted by birds and mammalia; and that the almost exclusively reptilian fauna of the lands whose zoological and botanical characters have formed the subject of this lecture, was but an exaggerated condition of that state of the animal kingdom which is exhibited by the present fauna of the Galapagos Islands.*

On the Clouds and Equatorial Cloud Rings of the Earth.†

By Lieut. MAURY, of the National Observatory.

Sailors have opportunities of making observations on clouds, and the various phenomena accompanying them, which no other class enjoy. The sailor, bound in his ship to the southern hemisphere, enters the region of the north-east trade-winds, and frequently finds the sky mottled with clouds, but generally clear; continuing his course south, he observes his thermometer to rise as he approaches the equator, until entering the equatorial region, he finds the weather to become murky, close, and oppressive. He then enters the south-east trades; and on looking at his log-book, he is

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* See "Wonders of Geology." Sixth Edition, p. 893.

† The above is an abstract of a paper read at the meeting of the American Association, Albany, by Lieutenant Maury.

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surprised to find that, notwithstanding the oppressive weather of the rainy latitudes, both his barometer and thermometer stood lower in them than in the clear weather on either side of them. In passing that rainy latitude, he has passed a cloud-ring which encircles the earth.

Lieutenant Maury then proceeds to give a description of the various changes which this great equatorial cloud-ring undergoes, and of its effects on the climate over which it hangs, the laws which control its shifting, sometimes to the north and sometimes to the south of the equator, and the accessions it receives from the more temperate latitudes, while the ring itself is the great source of supply of moisture to the regions of the earth very distant from the equator. Thus this cloud-ring modifies the climate of all places beneath it; overshadowing at different seasons all parallels from 5° south to 15° north. It may be asked, where do the vapours come from which are condensed and poured into the sea as rain? They come from the trade-wind regions under the cloud-ring, then rise up, and as they rise they expand, and as they expand they grow cool and are condensed. There is, therefore, a ceaseless precipitation going on under the cloud-ring. Evaporation under it is suspended nearly the whole year round. This ring is formed by the meeting of the NE. and SE. trade-winds; the vapours which each bring from northern and southern regions meet and ascend. Our knowledge of the laws of nature will tell us, therefore, that the atmosphere will be cooler under this ring than on either side of it, without consulting the thermometer. Were the clouds which overhang this belt luminous, and could they be seen by an observer from one of the planets, these clouds would present an appearance not unlike the rings of Saturn. He would also observe that this ring had an apparent movement contrary to that of the earth; for though it moves with the earth, the motion of the ring is relatively slower, and the earth slips from under it, giving the ring an apparent slow motion from east to west. This ring would be unlike those of Saturn in another respect: its edges would appear very jagged, and rough, and uneven.

Navigators are now learning to tell by the barometer when they have passed the cloud-ring. In the log-book of an American Captain in a voyage round the world, in 1850-51, recently forwarded to the National Observatory, I find the following remarks:—"I here predict," he says, before reaching the equator, "the barometer will remain below 30 in. until we get without the influence of the rainy latitudes." After having crossed a belt of five or six degrees of latitude, within which such remarks are frequent, as "Warm and sultry;" "Heavy rains;" "Very murky and close at times;" "Quite oppressive," "Rain," &c;—on the seventh day he remarks, "Assuming the settled weather of the trades, only requiring a rise of barometer to assure me of that fact." The day after, I find in his

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column of remarks, "Fine weather, every appearance of trades—barometer up." This remark is made the 5th March 1850, in 6° south lat. Had he passed this cloud-ring in August, he would probably have made the same observations in 6° north lat., indicating that he had passed from under the influence of this equatorial cloud-belt.

It is thus we arrive at a new application of the barometer, which thus informs the navigator, when other means fail, when he leaves and when he enters the trade-winds.

On the Blackheath Pebble-bed, and on certain Phenomena in the Geology of the Neighbourhood of London. By Sir CHARLES LYELL.*

There are two kinds of flint-gravel used for making roads in the neighbourhood of London, both of them in certain places superficial, but which are of extremely different ages. The yellow gravel of Hyde Park and Kensington so often found covering the "London Clay" may be taken as an example of one kind; that of Blackheath, of the other. The first of these is, comparatively speaking, of very moderate date, and consists of slightly rolled, and, for the most part, angular fragments, in which portions of the white opaque coating of the original chalk flint remain unremoved. The more ancient gravel consists of black and well-rounded pebbles, egg-shaped or spherical, of various sizes, exhibiting no vestige of the white coating of the original flints, yet shewing by the fossil sponges and shells contained in them that they are derived from the Chalk. In the pits of Blackheath and the neighbourhood, where this old shingle attains at some points a thickness of 50 feet, small pieces of white chalk sometimes occur, though very rarely intermixed with the pebbles. If we meet with thoroughly rounded flints in the more modern, or angular gravel, it is because the latter has been in part derived from the denudation of the older bed.

The researches of the Rev. H. M. De la Condamine have shewn that the sand and pebble-beds of Blackheath and Greenwich Park inclose in some of their numerous layers fresh-water shells of extinct species, such as *Cyrena cuneiformis*, &c., agreeing with fossils which characterise the Lower Eocene beds at Woolwich. At Lewisham the pebble-bed passes under the London Clay, and at Shooter's Hill this clay overlies it in great thickness.

At New Charlton, in the suburbs of Woolwich, Mr De la Condamine discovered a few years ago a layer of sand in the midst of the pebble-bed, where numerous individuals of the *Cyrena tellinella* were seen standing endwise, with both their valves united, the posterior extremity of each shell being uppermost, as would happen if the molluscs had died in their natural position. Sir Charles Lyell

* Read in Royal Institution of Great Britain, on April 1, 1852.

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described a bank of sandy mud in the delta of the Alabama river at Mobile, on the borders of the Gulf of Mexico, where, in 1846, he had dug out, at low tide, specimens of a living species of *Cyrena*, and of a *Gnathadon*, which were similarly placed, with their shells erect, a position which enables the animal to protrude its siphons upwards, and draw in water to lubricate its gills, and reject it when it has served the purposes of respiration. The water at Mobile is usually fresh, but sometimes brackish. Sir Charles examined lately the Woolwich beds with Mr Morris, and they verified Mr De la Condamine's observations, observing there several dozen specimens of the *Cyrena tellinella* in an erect position. From this circumstance the lecturer infers, that a body of fresh or river water had been maintained permanently on that spot during the Eocene period, and the presence of rolled oysters in the associated pebbly layers, with other marine shells, mixed with species of *Melanopsis*, *Melania*, *Cerithium* and *Neritina*, demonstrate that the sea occasionally invaded the same area. To an overflow of the pebbly sand in which the Cyrenæ lived by salt water, may probably be attributed the poisoning of the molluscs which left their shells uninjured on the spot where they had lived.

The stratum called "the shell-bed," which contains at Greenwich, Woolwich, Upnor, near Rochester, and other places, a great mass of fresh water, brackish water, and marine shells, especially oysters, is observed everywhere to underlie the great pebble-bed. Its mode of occurrence implies the entrance of one or more rivers into the Eocene sea in this region. Other rivers draining adjoining lands are indicated by a similar assemblage of fluvio-marine fossils near Guildford and at Newhaven in Sussex. The vicinity of land to the south and west of Woolwich is shewn by the occurrence at New Cross, Camberwell, and Chelsea, of *Paludina* and *Unio* in strata evidently a prolongation of the Woolwich beds, and by fossil leaves of dicotyledonous trees and layers of lignite in some of those localities. On the other hand, at the junction of the "London Clay," and the subjacent "plastic clays and sands," when followed in an opposite or easterly direction towards Herne Bay and the Reculvers, all signs of the fresh-water formation disappear, and the pebble-bed is reduced to a thin layer, often a foot or a few inches in thickness. The origin of this shingle may have been chiefly due to the action of waves on a sea-beach. Its accumulation in great force at certain points where fresh-water shells abound, seems to imply the entrance of rivers into the sea, which brought down some flints, and arrested the progress of others travelling as beach-pebbles along a coast-line, in a certain direction, determined by the prevailing currents and winds. The spreading of the pebble-bed over a wide area may be accounted for by supposing a gradual subsidence of land, and the continually shifting of the coast-lines upon which shingle accumulated. This same subsidence is required to explain the superposition of the

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London Clay, a deep-sea deposit to the Blackheath or Woolwich beds which are of shallow water or littoral origin. One of the rivers of the Lower Eocene period swept into the sea at Kyson, near Woodbridge in Suffolk, the bones of a monkey of the genus *Macacus*, of a marsupial quadruped allied to the opossum, of a *Hyracotherium*, and other mammalia, which have been determined by Professor Owen, and which throw light on the inhabitants of the land, at an era antecedent to the deposition of the London Clay.

Sir C. Lyell then exhibited some sections, recently published by Mr Prestwich,* illustrative of the geology of the environs of London, and gave a rapid sketch of the successive Eocene groups from the London Clay and overlying Bagshot series, with its nummulites to the Barton and Hampshire fresh-water formations, with their fossil quadrupeds. He then alluded to the tertiary strata next in the ascending order which he had recently studied in Limburg, Belgium, which are not represented in England, and next to the Miocene faluns of Touraine and the Pliocene strata or crag of Suffolk, and lastly to the still more modern glacial period and the brick-earth of the valley of the Thames. The last-mentioned formation contains the bones of extinct quadrupeds mingled with shells of recent species, terrestrial and fluviatile.

The numerous and important changes in the fauna of the globe, attested by these successive assemblages of extinct species, belonging to different tertiary eras, attest the vast lapse of ages which separate the time when the fresh-water beds of Woolwich and Blackheath were formed from the human period. But revolutions of another and no less striking kind have taken place contemporaneously in the physical geography of the northern hemisphere, revolutions on so great a scale that the greater part of the present continents of Europe, Asia, Northern Africa, and North America, with which the geologists is best acquainted, have come into existence in the interval of time here alluded to. It may also be confidently affirmed that the colossal chain of the Alps is more modern than the tertiary shingle of Blackheath. There was deep sea at the period when the London Clay was forming, precisely in the area where the loftiest mountains of Europe now rise into the regions of perpetual snow. In proof of this the lecturer referred to the works of several modern geologists, especially to those of Sir Roderick Murchison, and to a lecture delivered by Sir Roderick in the Royal Institution, to shew that the nummulitic formation which belongs to the Eocene period, and not to the very oldest part of that period, attains an elevation in some portions of the Swiss Alps of 8000 or even 10,000 feet, and enters into the structure and composition even of the central axis of the Alps, having been subject to the same movements and partaking of

* Prestwich, Geological Enquiry respecting the Water-bearing Strata around London, &c. Van Voorst, 1851.

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the same foldings and contortions as the underlying cretaceous and oolitic strata.

Sir Charles Lyell next proceeded to shew that a great series of volcanic eruptions had occurred in Europe since the older Eocene strata of the neighbourhood of London were deposited. Not only Vesuvius and Somma, as well as Etna and the extinct volcanoes of Southern Sicily, but the trachytic and basaltic eruptions of the extinct volcanoes of central France, are more modern than the London Clay. The evidence consists not only of the superposition of igneous rocks several thousand feet thick, to lacustrine strata of the middle and upper Eocene periods, but also to the absence in the pebble-beds constituting the base of the tertiary series of Auvergne, Cantal, and Velay of any pebbles of volcanic origin.

The lecturer concluded by stating that the formation of every mountain-chain and every elevation and depression of land bears witness to internal changes at various depths in the earth's crust. The alteration has consisted sometimes of the expansion, and sometimes of the contraction of rock, or of the semi-liquefaction or complete fusion of stony masses and their injection into rents of the fractured crust occasionally manifested by the escape of lava at the surface. Every permanent alteration therefore of level may be regarded as the outward sign of much greater internal revolutions taking place simultaneously far below. Even the precise nature of the changes in the texture of rocks produced by subterranean heat and other plutonic influences since the commencement of the Eocene period can be detected in a few spots, especially in the central axis of the Alps, where the disturbing agency had been intense. The table might be covered with specimens of gneiss, mica schist and quartz rock, once called primitive, and once supposed to be of a date anterior to the creation of living beings, which nevertheless were sedimentary strata of the Eocene period, which assumed their crystalline form after the flints of Blackheath were rolled into shingle, and even after the shells of the London Clay and the nummulites of the overlying Bagshot sands were in existence.

Yet however remote may be the antiquity of the Blackheath pebble-bed, as demonstrated by the vast amount of subsequent change in physical geography, in the internal structure of the earth's crust, and in the revolutions in organic life since experienced, its origin is probably as widely separated from the era of the Chalk as from our own times. For the fossils of the Chalk differ as much from those of the oldest tertiary strata near London, as do the last from the organic beings of the present era. Nevertheless the white Chalk itself, with its flints, is considered by every geologist as the production of a modern era, when contrasted with the long series of antecedent rocks now known, each formed in succession when the globe was inhabited by peculiar assemblages of animals and plants long since extinct.

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On the Great Principles either Suggested or Worked out by the late celebrated Dr William Prout, F.R.S., &c. By Dr DAUBENY, Professor of Botany, Oxford.* Communicated by the Author.

In noticing the advances made to our knowledge of the functions of life, through the instrumentality of chemistry, as illustrated by a new discovery of Baron Liebig's, of which he had given a short account, Dr Daubeny could not refrain from dwelling a little upon the scientific merits of an old and valued friend of his own, now deceased, who led the way in this path of research, and deserves to be commemorated, both for his important contributions to chemistry in general, and likewise for the light which his researches first cast upon many obscure processes of the animal economy. He alluded to Dr Prout, whose labours, however, in the cause of science, he would not take up the time of the Society by particularising, inasmuch as a pretty faithful and detailed abstract of his principal papers had already been given to the world in a late number of the *Edinburgh Medical Journal*.† He would, however, briefly allude to two qualities which eminently distinguished his philosophical character, and which, by their happy combination, enabled him to render subservient to the unfolding of grand general truths those minute pathological inquiries which his profession prompted him to undertake, but every one of which, when once entered upon, was worked out by him with the patience and exactness of a philosophical problem.

The first of these characteristics was that capacity for accurate observation, which, coupled as it was in him with the most conscientious regard to truth, inspired such a confidence in his published results, that their correctness has seldom been impugned by those who, with the lights of improved knowledge, have since followed in his footsteps. It is, indeed, the great boast of Liebig, that he has so improved the method of analysing organic bodies, that a young man of ordinary

* Read before the Ashmolean Society of Oxford, February 3, 1852.

† July 1851.

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attainments can now, after a few months' training, complete an analysis, which may be appealed to with confidence, and received as the basis of further research; whereas before, only adepts in chemistry were capable of bringing out results upon which any reliance could be placed.

Yet the greater part of Dr Prout's analyses were made with an apparatus of his own, which, however ingenious it might be, was far more difficult to use, and required for its success many more precautions than that at present in the hands of chemists, and hence the precision to which he attained is the greater subject for commendation. Add to which, that these delicate investigations were carried on by him, unassisted, amid constant interruptions, at intervals snatched from the daily demands made upon his time by professional engagements.

The second characteristic of his genius was that power of generalisation, that aptitude of combining into an harmonious whole, a number of isolated and independent facts, which led him to seize upon the remote consequences deducible from the results of his own observations, as well as those of others, and at the same time to shape his inquiries in such directions as might lead to the development of great principles in science. Thus, for instance, so far back as the year 1815, he published that remarkable paper, "On the relation between the Specific Gravity of Bodies in their Gaseous State, and their Atomic Weights," in which he pointed out, within a few years after the promulgation of the Daltonian theory, that the atomic weights of all other bodies may be regarded as multiples of that of hydrogen,—a position which, after being disputed by Berzelius and other great authorities, came at length to be confirmed with respect to three of the most important elements—carbon, oxygen, and azote—by the researches of Dumas and others.

Thus, at a later period, his delicate method of weighing the air enabled him to suggest a cause for the prevalence of the cholera then raging in London—namely, the addition of an ingredient to the ordinary constituents of the atmosphere, which increased the specific gravity of its lower strata over that locality.

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It may be mentioned, as a proof of that admirable caution which he evinced with regard to facts, even when tempted by the support they would have yielded to any of those ingenious speculations with which his mind was ever teeming, that although he was understood to have continued the meteorological researches alluded to during the whole period of the visitation of the cholera in 1832, he delayed their publication until they could be still further corroborated. Unfortunately, when the cholera broke out a second time, in 1848, his health was too much enfeebled to allow of his undertaking, in addition to a large medical practice, a similar course of laborious investigations, so as to satisfy his own scrupulous mind as to their truth.

Dr Prout also suggested an explanation of the differences existing between those organic bodies, whose constituents had appeared identical, by the interference of infinitesimal portions of certain extraneous substances intermixed with their predominant ingredients; and started the idea, which Liebig has followed up with so much success, that these latter may be of essential use, inasmuch as they render the body itself suitable to be assimilated by animals, owing to their counteracting in it those chemical affinities between its particles which would otherwise be too powerful for the antagonistic forces of life to surmount.

Substances so constituted he called *merorganised*, and the introduction of these foreign matters he regarded as the cause of that new arrangement of their particles which imparted to them properties altogether distinct from those which before characterised them. Thus, starch he regarded as merorganised sugar, and considered the latter body to be incapable of assimilation, until it had undergone an alteration of this kind within the body.

Dr Prout also led the way towards the establishment of that beautiful classification of substances subservient to nutrition which Baron Liebig has lately brought so prominently forward, and made the foundation of so many striking and interesting speculations. His paper "On the Ultimate Composition of Simple Alimentary Bodies" shews that they are divisible into three kinds, namely, the saccharine, the

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oily, and the albuminous, and likewise that the milk which nature has provided for the support of the young in mammiferous animals is alone capable of sustaining life, because it contains all three.

Thus, while the former inquiry of Dr Prout's contains the germ of one great principle so insisted upon by Liebig; namely, the necessity for those minute quantities of mineral matters which are found to be present in plants, the latter suggested the groundwork of the Baron's other great work, in which he has explained so luminously the nature of the proximate principles required for the nutrition, and for the maintenance of heat, in animals. With regard to inquiries more purely medical, Dr Prout first gave a clear idea of the constitution of the urine, and shewed that the secretion of urea took place in the bloodvessels, whilst it was merely eliminated by the kidneys. By ascertaining that the urine of reptiles consists wholly of uric acid, he took the first step towards pointing out the relation between that body and urea, which latter Liebig supposes to be produced in warm-blooded animals, through the oxygenation of the former compound.

While by this train of research he threw so much important light upon the physiology of calculus, and other urinary disorders, he advanced at the same time our knowledge of digestion itself, by his discovery that the stomach in a healthy state always contains free muriatic acid. Hence probably the necessity of salt for all the higher animals.

Such are a few of the great principles, either suggested or worked out by Dr Prout—contributions to physiological science important enough to establish his reputation as a great original thinker, as well as an accurate and scrupulous experimentalist. His two principal publications, namely, his *Bridgewater Treatise* and his work *On the Stomach and Urinary Diseases*, are each characterised by a very high, although a distinct order of merit. The former not only evinces a thorough mastery of the details of his subject, but also much ingenuity in unravelling the mysteries which beset us when we attempt to speculate on the intimate constitution

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of matter. While soaring into this elevated region, he caught a glimpse of those views respecting the distinction between *physical* ond *chemical* atoms, from the development of which Dumas has since derived so much celebrity.

On the other hand, in his latter work, dedicated to the relief of human suffering, he has abstained as much as possible from such speculations, and has evinced an exemplary caution in confining his practical deductions strictly within their legitimate limits, at the same time that he has displayed a profound sagacity in the discrimination and treatment of the diseases which fell within his province.

The Cambrian and Silurian Discussion.

In the fifty-second volume of this Journal we inserted, at page 305, from the *Literary Gazette*, Professor Sedgwick's classification of the Palæozoic Rocks, in which he describes the position and extent of the Cambrian and Silurian groups of that great series. Sir R. I. Murchison, in a letter to the *Literary Gazette*, inscribed at page 355 of the same volume (fifty-second volume) of this Journal, objects strongly to Professor Sedgwick's views.

The discussion did not terminate with this letter, for in the present number of our Journal we have to insert three additional letters since published in the *Literary Gazette*.

Were we not convinced that this discussion would form an interesting episode in the history of English geology, we might be blamed for occupying so many of our pages with controversial matter.

The following are the three additional letters :—

1. *Professor Sedgwick's Answer to Sir R. I. Murchison's Letter inserted in the Literary Gazette, and at page* 355 *of the Fifty-second Volume of the Edinburgh Philosophical Journal.*

Cambridge, 5th April 1852.

An absence from Cambridge during two weeks prevented me from seeing your '*Literary Gazette*' of March 20, at the time of its appearance; and it is through the kindness of a friend that I

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have become at length acquainted with Sir R. I. Murchison's comment on an abstract ('*Literary Gazette*,' March 6) of a paper lately read by myself before the Geological Society of London. Had this comment been merely an exposition of certain general views of classification or nomenclature which differed from my own, it would have passed on my part without any further notice; but it contains assertions that are founded in mistake, and, I believe, contrary to fact; and it ends with a poetical squib, which may perhaps, among persons more open to mockery than to argument, have helped to raise a minute's laugh against me. Squibs are, after all, but sorry arguments. They seldom promote the cause of truth, and they never minister to good temper. I think I know my poetical antagonist—*ex ungue leonem*—and I forgive the lion. His roar, like that of the illustrious Bottom, is as gentle "as the note of any sucking dove." I am told that he is "a profound naturalist;" and, if I am right in my man, this is true to the very letter, and beyond the letter. But has he ever cast a philosophical view over all the older rocks of Britain, so as to be a good judge on a general question of classification and nomenclature bearing upon the distribution of the oldest physical groups in this island? I might, perhaps, have answered this question in the affirmative, as a matter of belief or of courtsey, had I not read this poetical illustration of his mistaken nomenclature and narrow creed. He is a naturalist and a poet, and in this instance, more of a poet than a naturalist; for poets deal best in fiction. Martial's weapons are too fine for geology; if my poetical friend means to be a geologist, Vulcan's hammer will serve his purpose better.

When he tells me that "Silurian beds we in myriads number," I tell him, in reply, that in Cambria I can outnumber his beds when five times told, though I begin my reckoning *below* any rock which has ever had its place fixed in a *true Silurian section*; and when he adds that of "Cambrian strata *stat nominis umbra*," I retort upon him, that it is he who has put them in the *shade* by daubing his own mistaken colours over them. Let him leave them where nature placed them, and they will then shine forth in their true colours — the grandest, the best-marked, and most glorious objects of the whole British palæozoic series.

He, it seems, has been inverting Nature's history by reading her story backwards; by adopting a scheme in which he names the palæozoic ancestors after their palæozoic progeny, instead of the progeny after their ancestors; by building his garrets in the air before he has so much as thought of the lower stories of his fabric; or (to leave figures) by giving a name to the great Cambrian series, borrowed from a newer country in which that series is not found; and by vindicating this name by a "downward development," such as mocks the whole order of nature's laws and

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workmanship. Schemes of development and nomenclature worked out on such a plan must inevitably disfigure science by loading it with incongruous names; and, worse still, must hinder its true progress, by flattering a very mischievous spirit of premature generalization.

He twits me with "lagging behind" my best fellow-labourer and friend. It may be so; but I have not clung to the skirts of his garments, or hindered his progress; for we have not worked together among the Silurian and Cambrian rocks of England since 1834. But when the author of the squib *seems* to tell me that I am trying "to stifle" my friend to serve my own selfish purpose, he insinuates what is unjust to myself, and is unworthy of his pen. I have for thirty-four years kept my neck pretty steadily in the geological collar without ever having known a task-master, and for nearly thirty years I have devoted no small labour to the older palæozoic series, especially in the British Isles. For every year which the author of the squib may have toiled among these rocks, I believe that I have toiled ten; and whatever may be thought of the result of my labours, be it great or small, or nothing, this at least I do affirm, that I have stood in the way of no man, and that I have ever done my best to stifle any spirit of premature generalization that might rise within myself, lest it might minister to my personal vanity rather than to the lasting cause of truth. Hence I have never been over-anxious to give names to ancient groups of strata; and where I have used such technical names, I have willingly changed them when the occasion seemed to call for it; and by these very changes, made in deference to others, have I more than once been led into great errors of nomenclature. So far as regards the great Welsh series, I venture to affirm that, from first to last, my Cambrian sections were *right in principle;* and that I never misinterpreted my upper groups except when I endeavoured, hypothetically, to adapt them to the lower Silurian groups, which, in the end, were proved to be *wrong in principle.* When this was at length made out, it would have been an act of downright folly and moral cowardice not to adhere to my original classification of the great Welsh series. I believe my classification will stand, because it is but a transcript of nature's true progressive development; and that my nomenclature will stand, because it is geographically true, and is built upon the common-sense principles announced in the opening words of the sixteenth chapter of 'The Silurian System,' and because (though a matter of far less moment) my names have the right of priority. Herein I dare to "appeal to the sense of mankind"—not in the language of poetical mockery and fiction, but in plain prosaic words, which are the honest transcript of my belief.

To take away from that *umbra* which has eclipsed the vision of

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my poetical friend, I advise him to read my last geological paper, should it be printed, not by the glare of his own fireworks, but in the light of day; and then, should he think a single paragraph of it worth discussing, to discuss it with me in plain prose.

Before I notice one or two statements of my friend, which are undoubtedly erroneous, let me, shortly as I can, call the attention of the reader to some preliminary facts and principles which virtually settle the whole question in debate.

We began our labours independently in the summer of 1831. In July 1832, I exhibited before the British Association a section through the undulating system of Carnarvonshire, with the exception of a single doubtful group near the Menai. I then determined the true succession of the several subordinate groups of my section and I have never changed it since. During the same summer (1832) I completed one or two parallel sections, from the Menai to the edge of Shropshire, which determined, I believe, correctly the general relations of the whole Cambrian series of Carnarvonshire, Merionethshire, and Denbighshire. In the autumn of the same year I made two or three rapid traverses through South Wales, which, however imperfect as to details, enabled me to determine with absolute certainty that all, or nearly all, the eastern portions of the great undulating system of South Wales (lying to the east of Cardigan Bay) was superior to to the Bala limestone.

In 1833, my friend exhibited before the British association some of his best sections through a district two years afterwards called Silurian. I followed him with an explanation of my sections, above noticed, across the great series of North Wales. What was then the state of our knowledge, and how far were we agreed? At that time the overlying flagstones in the north of Denbighshire offered no difficulty. They were the undoubted equivalents of the overlying flagstones near Welsh Pool, afterwards called Upper Silurian. Neither at that time did the coarse sandstones and conglomerates at the base of the Denbigh flags offer any difficulty of interpretation. They appeared to represent, very naturally, the shelly sandstones, &c. (afterwards called Caradoc), of the sections exhibited by my friend. But there *was* a difficulty in the interpretation of my groups on the east side of the Berwyns. Left to my own sections I should, without hesitation, have placed these groups nearly on the parallel of the calcareous slates east of Bala, and called them a part of my Upper Cambrian series; but this conclusion seemed to clash with my friend's interpretation of the plan of some shelly sandstones (Caradoc) near Welsh Pool. Hence I concluded by asserting "that there must be an *overlap* of our sections on the east sides of the Berwyns, which could only be explained by a joint interpretation made by us in the field."

I have noticed the previous facts in historical order, mainly for

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the purpose of laying down a *principle of common sense and common justice*—viz. that if (in the region where the 'overlap' took place) I had blundered in my sections, and mistaken the relations of my upper groups, I was bound to expunge them from my Cambrian series, and give them up to my friend. On the other hand, if he had misinterpreted the relations of his lower groups (Llandeilo flags, &c.), while I had given their equivalents a right place in my Cambrian sections, he was bound, by the same principle, to give up those lower groups to me. In laying down this principle (in plain prose and not in the language of poetical mockery), I also "appeal to the sense of mankind."

My friend tells me (*Literary Gazette*, March 20, and at page 355 of vol. lii. of the *Edinburgh Philosophical Journal*) "that geologists must adhere to his nomenclature, founded on *data which have proved to be true*." I reply, that my position in this controversy is defensive, and not offensive; that I maintain the nomenclature first agreed upon; and that my friend's nomenclature cannot be now adopted, simply because the data on which it was constructed *have, out of all question, proved to be untrue*. In 1834 we visited together (and for the express purpose above mentioned) what we supposed to be the most typical Silurian country both of South and North Wales. And what were the results? The *base line* of the Silurian system, to the south of Welsh Pool, was then, I believe, laid down by my friend very nearly as it was afterwards published in his great work. I dictated not a single point of it. And this line was not laid down at random, but on the supposed evidence of sections, as interpreted by himself. Along the whole base line, so far as it was explained to me, the rocks he coloured as Cambrian were supposed by himself to be inferior to his lower Silurian groups; and that he did not change his views on this material point during the four or five years which followed, is demonstrated by many passages of his great work, among which I may refer to those found in pages 256, 317, 319, 343, 356, &c. &c. I then took this line on trust, and there was but one single point of it which we critically examined together. I *now know* that he misinterpreted his own transverse sections, and that he made a double mistake—first, in uniting the Llandeilo and Caradoc groups; secondly, in *making them both superior* to the rocks which in his great map are *coloured as Cambrian*. I made no mistake as to these Cambrian rocks; for on the evidence of my own section I simply affirmed that they were superior to the Bala limestone.

We then examined together the beds on the east side of the Berwyns. My friend pronounced the Meifod beds to be (as they were soon afterwards called) Caradoc sandstone, *in its most typical form*; and from these very beds he has derived some of the good

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Caradoc fossils which were figured in his work. On a Silurian question I believed him infallible; and on the principle of common justice above laid down, I struck out the calcareous slates, east of the Berwyns, from my Cambrian series, and coloured them as they afterwards appeared in the first great Silurian map. This I did without reserve, though I thereby threw my own upper Cambrian sections into inexplicable difficulties. How completely I was misled by this misinterpretation of my friend, and when, and by what means, I afterwards returned to right views, which confirmed my original sections, cannot be discussed here.

Finally, we visited the Bala limestone, and on the evidence of sections (though a single eye-glance was enough to shew that the Bala fossils were very nearly the same with those of Meifod), my friend accepted my interpretation of its geological place, and, spite of its fossils, pronounced it to belong to an *undoubted Cambrian, and not a Silurian group*. And the conclusion, so long as he maintained the integrity of his own lower Silurian sections, was irrisistible. If it has since been proved that the Llandeilo flag is the equivalent of the Bala limestone, there arises this question, By whose mistake were these two groups ever separated? I reply, they were kept asunder by a great fundamental *mistake* in the Silurian sections, and by *no mistake* I ever committed in my Cambrian sections. This mistake is stamped on my friend's great map, as well as on a page of his first paper that was written after there arose a controversy between us.* Here, therefore, I apply the principle (above laid down) of common sense and common justice, and claim the Bala limestone and the groups immediately above it, and all their equivalents wherever found, as true and integral parts of the upper Cambrian series.

In the sense in which our author first used the words Silurian System, his nomenclature was not only premature, but erroneous. For the fundamental sections on which his nomenclature was grounded were untrue to nature; and we cannot have a true system that is built upon a false base. I think I may again "appeal to the sense of mankind" in vindication of this last con-

* See *Journal of the Geological Society*, 1847, p. 167. The section given on this page is, in fact, the foundation of the whole Silurian nomenclature. But my friend's statement is entirely incorrect when he adds, that its lowest beds were intended only "to represent certain inferior unfossiliferous rocks, such as those of the Longmynd." The section does not at all apply to the case of the Longmynd; and the inferior (or Cambrian) rocks of his original sections *were not considered unfossiliferous* either in 1834 or in 1839, when his System was published. He knew the contrary. And when he adds, that his sections were meant to represent what is stated in this page (167), he does not give his original interpretation of them, but he entirely *shifts his ground*, and puts upon them a *new meaning*, in order to bring them into conformity with a new map founded on a new scheme of nomenclature.

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clusion. Had the author (when he found that his fundamental sections were wrong) contracted his system, and based it on the Caradoc group, which seems to be a natural connecting link between the true Silurian and the true Cambrian groups, he might have continued his nomenclature, and maintained it in its true integrity. For the several groups of his system would then have been well defined "by the order of superposition and imbedded organic remains" seen in a series of true typical sections; and the collective groups of his system might have been very properly called Silurian, "to mark thereby (using his own words) the territory in which the best types and clearest relations were exhibited."

But in 1843 my friend applied a *new principle* of nomenclature to a great series of Cambrian rocks which he had never examined, and of which I had first determined the true general relations. They were to be called and coloured as Silurian, because they contained certain fossils common to the beds he had called Lower Silurian, yet of which he had, in his fundamental sections, misrepresented the relations. This principle I have a right to call new, for it was in direct antagonism with his conclusions in 1834, when we were together in the field. It was not enunciated in the "Silurian System;" it was not acknowledged, but rather contradicted, by what I had myself written, and it was never communicated to myself. I was no consenting party to the colours placed by my friend on a geological map of England in 1843; nor did I even know of its existence till two or three years afterwards. We may apply this principle with safety, if it be derived from an old system which has been perfectly defined, and of which the several subordinate parts have been already named; but as applied to a new and unknown system, such as that of Wales, it virtually destroyed the sense and meaning of the whole Silurian nomenclature, for it deserted the principles the author himself enunciated for his own guidance and the vindication of his adopted names. Palæontology is not the mistress but the handmaid of geology; and any new system, drawn from an unexplored country, is utterly worthless if it rest not fundamentally on the evidence of natural sections and natural groups. Fossil evidence may then follow, but it tells us nothing in a new system while the groups are undefined.

Out of all comparison the greatest boon, within my memory, conferred on palæozoic geology was the establishment of the Upper Silurian groups. The honour derived from this great boon is my friend's undisputed right. The establishment of the Devonian series soon followed by an almost inevitable necessity. We had long known, through many published works, the general aspect of the older palæozoic fauna; and when the Devonian and Upper

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Silurian species were removed, it was plain that we had no right to look for, nor did I *ever expect to find*, any great succession of changing organic types in the vast series below the Caradoc sandstone. But we have no right to name this vast series from a single group near its upper surface. My names are geographically true, and from the first were honestly derived from sections traversing the series from top to bottom. My friend stopped short, or mistook his way, in the descending series, and then ventured to blot out the old and true name, and to give his own name to a great series he had not explored, thereby violating a principle which teaches us that systems and groups must be established first, and that names must follow afterwards.

In another sentence of his comment, my friend tells me "that these observers (viz. Sir H. de la Beche and the other gentlemen of the Government Survey) have satisfied themselves that the region called Cambria, at a time when none of its fossils were described, is *made up of the same strata, and contains the same organic remains, as the lower Silurian rocks*," &c. &c. It is not correct to say that none of the Cambrian fossils had been described; but I will let that mistake pass, as a small matter in comparison of the enormous misstatements (to be explained only by the incautious hurry of my friend's comment) that the older Cambrian groups are *made up of the same strata, and contain the same organic remains as the lower Silurian rocks!* I would venture to stake my life upon the issue of a question as to the correctness of this assertion. I will give my friend a descending point two or three thousand feet below any rocks he has reached in any true section, and the gentlemen of the Survey will tell him that he may descend 20,000 feet lower still before he reaches the lowest limit of organic life as seen among the older groups of Wales.

That the Government surveyors have adopted my friend's nomenclature is true. But I believe that the director of the Survey adopted it, not because he thought it best and most true to nature, but because he believed "that I had given up a very good nomenclature." I believe he was led into this mistake by a map (first Number of the *Journal of the Geological Society*, p. 22), which was introduced by myself, but not submitted to my revision, and which, in the explanation of the colours, utterly misrepresents the meaning of my paper, and of which I therefore disclaim the authorship. Be this as it may, no authority upon earth can make the adopted nomenclature either historically just or geographically true.

I cannot follow my friend in his excursions to distant lands. The question between us is a question only of the classification of British rocks, and must be decided only on British evidence. If he, in a rash zeal for a premature nomenclature, has

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been misled himself, or misled others, in giving wrong names and wrong British equivalents to distant regions, such mistakes belong not to the fundamental questions discussed in this reply. I have looked only to facts and first principles, and *fiat justitia*, without regard to my own mistakes or those of others, shall be my motto. And I reaffirm that no authority on earth can make the lower Silurian sections right, or subordinate to them the great Cambrian series.

Had I space for the discussion, I could prove that several of the authorities quoted against me do, when rightly interpreted, make good weight of evidence on my side; and I have not the shadow of a doubt that my own scheme of classification will bring the older British palæozoic groups into far better co-ordination with the magnificent palæozoic series of America than they have ever been brought before, through the intervention of the imperfect, and (so far as regards the lower groups) the erroneous sections of the Silurian system. The comparison of foreign palæozoic rocks with those of Britain has hitherto, I affirm, not been based "upon a natural British arrangement" but upon an arrangement partly defective and partly erroneous, and therefore unnatural. The establishment of a better nomenclature, based upon better sections, will give foreign geologists better terms of comparison, and thereby clear away many existing mistakes, and much present confusion. Harmony and order will inevitably follow the establishment of a true typical palæozoic series in England; and the points at issue between my friend and myself by no means are dependent upon any future questions which may arise respecting certain changes in the palæozoic names and colours of foreign maps, provided my scheme of classification and nomenclature were adopted. The only questions admissible in the debate are those which have a bearing on the truth of the sections, the natural succession of the groups, and the geographical propriety of the collective names when applied to British rocks. With the assertion of this principle of common sense I conclude my reply.

ADAM SEDGWICK.

2. *Sir R. I. Murchison's Comments on Professor Sedgwick's Letter* (*No.* 1.)

April 17.

The letter of my old friend Professor Sedgwick, has not, it appears to me, at all affected the integrity of the Silurian System, as defined by myself many years ago, and as since understood and received by geologists. A very brief review of data which my friend seems to have forgotten, is alone required. It is beside the question now at issue to revert to what we respectively did in the field in 1831 and

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1832, or to appeal to what he communicated verbally in the latter year respecting a part of North Wales, the only printed record of which is comprised in about twenty lines of the first volume of the *Transactions of the British Association*.

The first methodical and digested view of a sedimentary succession beneath the known and fixed horizon of the old red-sandstone, was presented by myself to the Geological Society in 1834,* as the result of memoirs of 1831, 1832, and 1833. The four formations described then as "fossiliferous greywacke," were, in the year 1835, named the Silurian System, the two superior (Ludlow and Wenlock) being termed Upper, the two inferior (Caradoc and Llandeilo) Lower Silurian, and each being characterised by its fossils.

In 1836 the word Cambrian was first used, Professor Sedgwick affirming that the slaty rocks which he so termed, and which laid to the west of the Silurian region, *were all of them inferior to the strata of my system*. This inferiority of position has proved to be a fundamental misconception, as now demonstrated by the physical researches of the Government geological surveyors. In their hands the Cambria of Sedgwick, which was undefined and unknown through any publication of its fossils, has proved to be identical in age with the original published Siluria of Murchison.

In 1838, when the detailed descriptions of the composition of the Silurian System were published, I spoke of the line of demarcation on my map, as being provisionally set up between the Silurian rocks with which I was well acquainted, and a Cambrian series of which I was ignorant. But the latter being said to be vastly inferior to all the strata I described, I naturally believed it would prove, in the hands of my friend, to contain a distinct system of former life. Strata, identified by their fossils and infraposition to a known horizon, were the bases of my classification and nomenclature; and no other idea ever crossed my mind than that the Cambrian could alone be established as a system, by having a fauna different from my own, and by being inferior to it. Various passages in my original works clearly expose this view.

My friend says, that in 1843 I *shifted* my ground, and put a new meaning upon my views, in order to bring them into conformity with a new map founded *on a new scheme of nomenclature*. The recorded facts, he must forgive me for saying, are quite opposed to this assertion, and are indeed well known to practical geologists. The little geological map of England and Wales, published by me in 1843, at the request of the Society for the Diffusion of Useful Knowledge, was not issued "*rashly*," but after much deliberation and examination.

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* Proceedings of the Geological Society, vol. ii. p. 13.

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Having ascertained, in the years 1840 and 1841, that, on the Continent, the *Lower Silurian fossils were the lowest fossil types*, I traversed the North Welsh or Cambrian region in 1842, accompanied by one of my Russian coadjutors, Count Keyserling, to ascertain if a similar distribution prevailed in Britain. We satisfied ourselves (leaving all physical proofs to the Government geologists, who were then beginning their survey of Wales) that after many apparent flexures, strata containing the same fossils appeared on the flanks and summit of Snowdon, as those which we had left in Shropshire, and to the east flank of the Berwyn mountains, a country which had been specially mapped and described as "Silurian." It was, therefore, *after* foreign comparisons, and *after* an actual traverse of the so-called Cambrian region, that I published the map of 1843, which, though complained of by my friend, has proved to be correct, and substantially in general harmony with the final results, physical and geological, of the Government geological surveyors.

I would further refer to the unambiguous printed declarations with which I opened both my discourses of 1842 and 1843, as President of the Geological Society, to prove that I took every opportunity of publishing my conclusions *before* I issued that map. This, for example, is a small portion of what I printed in 1842,—"The base of the Palæozoic deposits, as founded on the distribution of organic remains, may be considered fairly established; for the *Lower Silurian is thus shewn by Professor Sedgwick himself* (I was then speaking of a recent memoir of his own) to be the oldest which can be detected in North Wales, the country of all others in Europe in which there is a great development of the inferior strata."*

As Professor Sedgwick made no opposition to this induction of the author of the Silurian System, nor to another ample illustration of it in 1843,† nothing, it seemed to me, remained to be done in British Palæozoic classification, except that the Government geologists, who were preparing detailed maps and sections of Wales, should decide whether there were, or were not, fossiliferous strata occupying a lower position than any which had been formerly described as Lower Silurian. Their reply, in a stratigraphical and physical sense, is what I affirmed in my last letter, not *rashly*, but after a careful reference to the maps and sections they have prepared. Taking the tract east of the Berwyn mountains, which I have described as *the* country of my region, which afforded the fullest development of Lower Silurian rocks resting upon unfossiliferous greywacke, these geologists have proved that the strata

* Proceedings of the Geological Society of London, vol. iii., p. 549.

† Ibid., vol. iv., p. 70.

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containing fossils, which lie between the Berwyns and Snowdon, (the Cambria of Sedgwick), are the same as those I had drawn in sections, and described in words, in the "Silurian System."

If any one wishes to verify this statement, let him compare my original coloured map and sections* of the country west of the Longmynd (Stiper Stones and Shelve), with those detailed maps and diagrams of the surveyors, which explain all the flexures and breaks across North Wales, and he will then see that our hard-working and able contemporaries have demonstrated that nearly all the fossil-bearing strata of Cambria have their equivalents in Siluria; and that even the rocks of unfossiliferous greywacke, to which Sir H. de la Beche and his followers now restrict the word Cambrian, are more copious and thicker in the Longmynd of my original tract of Shropshire, than in any of the similar North Welsh masses which underlie the strata of Snowdon. This is not my statement, but the deduction of the Government surveyors, after many years of labour in the field.

Supported as I am by such authorities, I can afford to be criticised for two or three *mistakes* in a large work containing about 240 coloured sections, woodcuts, and views. Relying on the impartial judgment of numerous contemporaries, who know how hard I laboured, and what I did effect in classification, I will not dispute about a patch of Caradoc sandstone here, or one of Llandeilo flag there, since I have laid it down that these subdivisions are characterised by many of the same fossils. The researches of late years have, indeed, confirmed the unity of the Silurian System, by shewing *that many of the same species of fossils pervade the whole series of its lower and upper rocks.*

It is this fact which prevents the possibility of a change of nomenclature, and the application of two unconnected names to *one system of life.* In short, the proposed amputation of the lower half of the Silurian System would, in my opinion, be a violation of nature.

In the work, *Russia and the Ural Mountains*, in which a general view of the whole ascending order, from the lowest to the highest strata, was given by my colleagues and myself, it was shewn that the Silurian System, so thick in Britain, has in the north of Europe so thin a vertical development (though equally divisible into very rich fossiliferous lower and upper deposits) as to be quite incapable of separation into two rock systems. But as Professor Sedgwick does not like foreign parallels, I will conclude this letter by citing two home *geographical* illustrations of the effect of his proposed nomenclature. Let any one cast his eye over my map of the

* "Silurian System," map, and pl. 31, fig. 4; pl. 32, figs. 1, 2, 3, &c.

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Silurian region, and he will see, that if the Lower Silurian rocks, which are represented as ranging through Brecon and Caermarthenshire by Llandovery and Llandeilo, be converted into Cambrian, the Silurian System (hitherto the only published type of comparison) will be reduced to an almost imperceptible line. Again, let the inquirer look in the same map at Broad Sound, in Pembrokeshire, where, to my great satisfaction, I first found all the Silurian formations, from the Ludlow to the Llandeilo, rising out from beneath the old red-sandstone, and then tell me if *fiat justitia* will reconcile geologists to the abstraction of the half of what I have proved to be Silurian, leaving me in a corner of one bay with my *Upper Silurian rocks only*—the *pars pro toto?* This must be the practical *British* issue of the adoption of the nomenclature of Professor Sedgwick.

The geologists and naturalists of the Government Survey, being satisfied that there is but one series of rocks and fossils in the bay of Pembroke above cited, have further proved, by admeasurement, that this same "Silurian System" is spread over nearly all Wales. To those persons and their works I again refer my friend. Establishing the Silurian System, I applied it to some foreign countries. The Government surveyors have spread it over Cambria, not through any *mistake* whatever, but on the true principle of assimilating things unknown to things which have been described.

I now terminate on my part a controversy which has given me much pain, yet from which I could not shrink; for mine is not merely a combat *pro aris Silurianis*, but the defence of a classification which I believe to be natural and indestructible. And although my old friend has, both in his first abstract (see *Literary Gazette*, March 6) and his last letter, used some racy expressions, and that I have thought it right to speak plainly, I look with undiminished confidence to our sliding down the shady slope of life with that mutual esteem and regard which were formed when climbing many a hill together both at home and abroad.

In our general geological views we are as united as in days of yore.

RODERICK IMPEY MURCHISON.

3.—*Professor Sedgwick's Reply to the preceding Letter of Sir R. I. Murchison.*

Norwich, May 8.

SIR,—My copy of the '*Literary Gazette*' for April 24 having been addressed to me at Norwich, did not reach me until my arrival here the early part of this week; and now (after having been several days indisposed and quite incapable of writing) I send my final answer to the last Silurian comments of my friend Sir R. I. Murchison. My replies shall be as specific and as short as

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I can make them; and I am writing from memory, without a single geological work before me, or a single note of reference.

1. "It is beside the question (he tells me, '*Literary Gazette*,' p. 369) to revert to what we respectively did in the field in 1831 and 1832." It is not beside the question to have done this. The comparison of our work (at the British Association in 1833) led us, by agreement, to a joint examination of the typical Silurian country in 1834. My friend had most perfect fair play. I did not contest his base line at a single point. On a Silurian question I believed him infallible; and I accepted his interpretation of the sections not only in South Wales but also in my own country on the east side of the Berwyns, where he pronounced the Meifod beds to be his *most typical form of Caradoc sandstone*, then called Shelly Sandstone. It was not I that cut away the Cambrian rocks from the Silurian. It was he that cut off the so-called Silurian rocks from the general system of North and South Wales, and declared them to form a *distinct and superior system*; and as such he described them in his sections, and afterwards coloured them in his map. On no other hypothesis could he give any real meaning to his nomenclature.

2. In the next paragraph he adds that all the rocks I called Cambrian, and which lay to the west of the Silurian region, were *affirmed by myself* to be inferior to the strata of his own system. I reply that I cannot consent to have the load of my friend's mistakes thrown upon my shoulders. If there be a single true Silurian rock on the west side of his base line, he has only himself to blame for the fact; the mistake is his, and not mine. But taking the Caradoc sandstone as the physical base of the Silurian system, we may still affirm that all the rocks to the west of the true Silurian base *are inferior to the whole Silurian system*. I contend that the Llandeilo flag (which is but one single stage in the Great Bala or Upper Cambrian group) is a Cambrian, and not a Silurian rock. I determined the place of its equivalent (the Bala limestone) correctly in my sections. My friend utterly mistook the true relations of his Llandeilo flag. Of this I had, as I thought, good evidence in 1846, when I revisited the Silurian country; and taking the new evidence given by the admirable details of the Government map, we may see at a glance the extent of the mistakes made by him in the interpretation of his Lower Silurian groups. (1.) The want of conformity of the upper to the lower groups is not brought out in the Silurian map; and this led to a mistaken interpretation of the next inferior group. (2.) The upper part of the Llandeilo group is mistaken for the Caradoc sandstone. (3.) The Llandeilo flagstone is made the base of the system, and is put in every section *above the undulatory rocks on its western side*. There is not one single section in South Wales

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where my friend has determined the true relations of the Llandeilo flag to the beds above it and below it, so as to define its place in a general section of Wales, whether real or ideal. I offer no criticism "*on two or three mistakes*;" I affirm that the whole conception of the relations of the Llandeilo flag to my Upper Cambrian group was erroneous; and that all the lower parts of my friend's general and ideal sections—the very foundations of his Silurian nomenclature—were wrong in principle.

3. In the same paragraph my friend adds, "that the inferiority of position (viz. of the Upper Cambrian or Upper Bala group to the Llandeilo flag) has proved to be a fundamental misconception." True; but with whom rests the blame of this fundamental misconception? Any man of common sense reading this paragraph, must conclude that the mistake was mine. But what is the fact? I made no mistake whatsoever when I affirmed that what I formerly called my Upper Cambrian system *overlaid the Bala limestone*. That the same groups *of Upper Cambrian strata underlaid the Llandeilo flag*, was the "fundamental misconception" of my friend; and he must bear the blame of it. The mistake I made was the adoption, during fourteen years, of this fundamental error on the sole authority of my friend.

4. At the end of the same paragraph he adds as follows: "In the hands of the Government geological surveyors, the Cambria of Sedgwick, which was undefined and unknown through any publication of its fossils, has proved to be identical in age with the original published Siluria of Murchison." Any plain, unsophisticated reader must conclude, I think, from such words as these, that in the interpretation of the Cambrian sections I had made some great radical mistake, and that the sections of my opponent were immaculate. Now the very reverse of this is the case. The Government surveyors have discovered no great fundamental mistake in my Welsh sections, while they have completely upset the scheme on which the two Lower Silurian stages were constructed by my friend. To say that the great Cambrian groups are "*identical in age* with the original published Silurian rocks of Murchison," is so extravagantly inaccurate, that it is no easy matter to describe its inaccuracy in respectful language. My friend might just as well affirm that all the vast series of rocks west of the Berwyns are *identical in age* with the Bala limestone! But we may ask, how have the Government surveyors brought the Llandeilo flag of the Silurian system into comparison with the great groups of Cambria? By a process of development, both upwards and downwards, by adding three or four thousand feet of strata (which had been completely misinterpreted by my friend and antagonist) *above* the Llandeilo flag, so as to connect it with the Caradoc sandstone, and then by adding, in a descending sec-

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tion, at least 25,000 feet to its base. The Llandeilo flag, thus developed and tricked out, becomes the equivalent of my Cambrian system! But under no interpretation, compatible with the plain meaning of words, can it be called "the original published Siluria of Murchison."

5. I re-affirm, with great confidence, that my friend has utterly shifted his original ground of classification. In his reply to my remarks on this head, he has kept out of sight the important fact, that in 1834 (the last time we were together in North Wales) he accepted my interpretation of the Bala limestone; and, spite of its fossils, declared his conviction that, by the evidence of the sections, it was unequivocally a member of the Cambrian series, and removed it out of his Silurian system. If he afterwards saw reason to change his views as to this essential point, he was, I think, called upon to communicate that change to myself; but no such communication was ever made to me. Again, it is by no means correct to say that the Cambrian rocks were undefined, and their fossils unknown. The rocks *were well defined* by true sections. No good general list of their fossils had been published by myself; but I stated, again and again, *before* the publication of the Silurian system, that many of the Cambrian fossils were of identical species with the Lower Silurian; that in the Bala group several fossils (enumerated by their specific names), and nine species of Orthis, were identical with known Lower Silurian species, &c. &c. *Lastly*, my friend himself, though he called the Bala limestone Cambrian, did not discover in it a single species that was not also found in the Llandeilo group. When he afterwards, discarding the evidence of sections, began to feel his way downwards, and, by help of *fossils only*, endeavoured to bring the great Cambrian groups within the narrow limits of his two Lower Silurian stages, I have a right to affirm that he shifted his ground, and deserted the original principles of his classification.

6. Speaking of himself and Count Keyserling, he informs us—"that in 1842 they satisfied themselves that, after many apparent flexures, strata containing the same fossils appeared on the flanks and summit of Snowdon as those they had left on the east flank of the Berwyns, a country which had been specially mapped and described as Silurian." As to the country east of the Berwyns, a part of it was erroneously mapped by my friend, and another part of it was erroneously coloured by myself, in conformity with his misinterpretation of the deposits, which he made Caradoc sandstone. To have been consistent, therefore, he and Count Keyserling must have regarded the top of Snowdon as Caradoc sandstone, a conclusion which would have been incontestably erroneous. As to the Snowdonian fossils, I had published a pretty good list of them at least twelve months before the summer of 1842, and a short list had been previously given by Professor Phillips. They

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were all of them species of the Bala group; but, unquestionably, that did not prove them fossils of any true Silurian stage.

7. As to evidence derived from foreign regions, I by no means protest against the reasonable use of it; but I do protest against its use in determining the proper fundamental nomenclature of British rocks. Questions of this kind must be decided on British evidence; neither can I now (from utter want of documents) discuss what my friend calls *induction*, made in 1843. At that time I did believe that my friend's lower Silurian groups would be found to descend as far as the Bala limestone—that the Bala limestone was Caradoc—and that a considerable part of the undulating groups of South Wales would turn out Upper Silurian. How I came, in 1843, to entertain, hypothetically, these erroneous views, and how I got rid of them, are questions I have discussed in a paper which before this time is probably published in the *Quarterly Journal of the Geological Society*.

8. *Lastly*, I request the reader to bear in mind that my whole position is not aggressive, but defensive. I continue to use the nomenclature mutually agreed upon about sixteen or seventeen years since. The rocks found in Wales, and not in Siluria, I still call Cambrian. All the rocks in Siluria are still called Silurian. My scheme involves no geographical incongruity, deprives my friend not of one single rock he is entitled to name on British evidence, and it has the incontestable right of priority. It does not exclude the Llandeilo flag from the rocks of Siluria. But this very Llandeilo flag (the Bala limestone) was in 1834 placed by my friend among the Upper Cambrian groups, and in a region where the sections were unambiguous and rightly interpreted; while the typical Llandeilo flag of South Wales was utterly misinterpreted by himself as to its geological relations. All this is clear to demonstration; and the conclusions that follow from it are inevitable, and settle at once all grounds of difference between my friend and myself. No man living can have a right to change his nomenclature by fashioning it to sections which are wrong, while he discards sections which are right.

In all his argument he seems to think especially of the importance of perpetuating his own premature nomenclature of foreign rocks; and this view warps his whole argument. His premature nomenclature is to be vindicated at whatever cost, and without any reference to the published sections of a fellow-labourer, who had honestly worked through the vast and most difficult series of true Cambrian rocks, and knew their relations; while he had himself barely touched the same series on its outskirts—had only pretended to describe two of its highest stages; and, as to the lower of these two stages, had irremediably blundered. Hence he has been compelled to adopt a monstrous scheme of development, leading to the monstrous conclusion that his single stage—the Llan-

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deilo flag—is, by this new scheme of development, identical in age with beds at least 25,000 feet in thickness, which undulate between the Berwyns and the Menai! And a monstrous development is to be followed by a new and monstrous nomenclature, in which a great fundamental group which does not exist in Wales is to be called Cambrian, while the grand series of the older Cambrian rocks are to be called Silurian, though they form the mountains of Wales, and do not exist at all in the typical Silurian region. No authority, however great, can perpetuate such an incongruous and unwarranted nomenclature; and it is historically as unjust as geographically it is incongruous.

From the very first, the term *Silurian System* was used prematurely by its author, and against repeated remonstrances on my own part. For the system had no clear physical or zoological base. Many of the fossils found below it in the Cambrian rocks, I affirmed to be of the same species with those in the two lower so-called Silurian groups. Advancing knowledge strengthened these objections, and it was at length discovered that the whole system, according to the author's scheme, rested physically on a false base. But there is still a good Silurian system, agreeably to my friend's use of the word System, based on the Caradoc sandstone. This sandstone is a great mechanical and often an unconformable deposit, constituting the true connecting links between Cambria and Siluria. It partakes of the zoological characters of both regions. At May Hill its fossils seem to be those of the Wenlock stage. At Horderley they very nearly approach to those of the Bala stage. Is there any single Caradoc section where these two fossil groups appear together? If so, are the types blended or superimposed? Some questions of this kind require a careful re-examination; though they have already been excellently handled by Professor Phillips. If questions such as these were completely solved, we should, I think, need nothing more for a full history of the true sequence and natural progressive development of the oldest fossil-bearing rocks of Britain, beginning with the Cambrian and ending with the Silurian series.

To the concluding words of my friend's comment (*Literary Gazette*, April 24, p. 370), I express my heartfelt concurrence. When we went round the Highlands of Scotland in 1827, I was then his superior in physical endurance; but a quarter of a century has, alas! made me but a sorry labourer in the field. Still I am not without hopes of again meeting him in his true Silurian country, and endeavouring to settle, along with him, one or two minute, and not laborious, questions of demarcation to which I have just pointed.

ADAM SEDGWICK.

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On the Ethnography of Akkrah and Adampé, Gold Coast, Western Africa. By William F. Daniell, M.D., F.R.G.S., Assistant Surgeon to the Forces, &c. Communicated by the Ethnological Society.

(Continued from vol. lii., p. 303.)

Deaths.—Upon the death of any native, several curious and interesting rites are strictly enjoined, from the performance of which they seldom deviate. Apparently great consideration is attached to them, if we may judge from the peculiar customs and celebrations enacted on such occurrences, not only by the inhabitants of the Gold Coast, but in most of the countries of Western Africa. Shortly after life has become extinct, the body is thoroughly washed by the household women, and every portion of it well rubbed over with a ligno-resinous powder named *teufan*, procured from the bark of a certain tree, which possessing an aromatic fragrance, is first pulverised and then appropriated as a perfume of ordinary use. The head and face are next carefully shaved, the limbs invested with their usual bracelets and other golden ornaments, and the whole body enshrouded in a number of the richest and most sumptuous dresses that can be chosen. If the deceased has been a person of consequence, gold dust is liberally sprinkled over the face and other uncovered surfaces, on which it is retained by the previous application of Ashanté grease or the vegetable butter, brought from the interior. The corpse thus arrayed is then exposed in state for a brief period for the farewell inspection of all relatives and friends, and is subsequently enclosed in a wooden box, and privately interred. In Akkrah the dead are invariably buried in one of the compartments within the house, but the slaves, unless they are favourites, lie scattered around the environs of the town, in some convenient spot selected for the purpose. Within the coffins of the more affluent are deposited a great variety of native cloths, gold rings, and other valuable trinkets, and occasionally a few bottles filled with gold dust, while upon their exterior surface are placed the brass ewer and basin, with the spoon, which the defunct was wont to employ during lifetime, and which the family now deemed an indispensable accompaniment towards the comfort to be attained in the next world. Until within a recent date, the immolation of human victims at these obsequies was fully authorised by the institutions of the country, to the end that the deceased might not be found deficient in the requisite number of attendants as would be found compatible with the rank he was supposed to keep in another sphere of existence. It is not many years since, upon the death of one of the powerful caboceers of Kinka, this sacrifice was consummated by the offering of two young slaves, who were slaughtered without compunction on the edge of the grave, and their bodies separately

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extended, the male below and the female above the remains of their late lord. Latterly, owing to the strict surveillance of the British Government, these barbarous rites have been temporarily abolished; but there can be little doubt that should these people ever become emancipated from the jurisdiction of Europeans, they would again revert to the observance of what to them is viewed in the solemn light of a sacred obligation.

On these melancholy occasions the wives and other near female relatives lament, in pathetic terms, their unfortunate bereavement, and affect to deplore, by external manifestations of grief, the irreparable loss they have sustained. The hair is totally shaven from the head, every ornament and personal decoration removed, and dark and sombre garments substituted in lieu of their ordinary dress, whose gayer hues were more emblematic of the cheerful days of the past, than of the gloomy prospects of the present. To evince the sincerity of their grief, the women studiously observe a solemn fast, abstain from every kind of food throughout the day, withdraw from public life, and immure themselves privately within the recesses of their respective chambers. For the space of three weeks or more, during the continuance of the custom that invariably succeeds, these injunctions are unequivocally obeyed, after which a certain degree of laxity follows, and the confinement of the wives becomes less restricted, they being permitted to frequent other divisions of the house and court-yards, and should circumstances compel an exit from their seclusion, a grave decorum is still preserved, and those conventional precedents that denote the mournful character of the duties entailed upon them, are carefully exhibited. The partial or entire removal of the hair, as a native testimony of affliction and sorrow, is one of those remarkable peculiarities that bear a close affinity to the ordinances introduced by the Jewish legislator in the 21st chapter of Deuteronomy, in which it is duly enjoined as follows:—

"Then thou shalt bring her home to thine house, and she shall shave her head and pare her nails. And she shall put the raiment of captivity from off her, and shall remain in thine house, and bewail her father and mother a full month."

That this was a usage of great antiquity, and common to many nations from the earliest ages of the world, long previous to its dissemination among the Jews, may be distinctly affirmed. Mention has been made of its prevalence by Herodotus, who relates that "it is elsewhere customary in cases of death, for those who are most nearly affected, to cut off their hair in testimony of sorrow: but the Egyptians, who, at other times, have their heads closely shaven, suffer the hair on this occasion to grow."* It was also equally practised by the Greeks upon the intelligence of any public or pri-

* Lib. 2, c. 36; *vide* also l. 6, c. 21.

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vate misfortune, the women clipping their hair short, and the men allowing it to grow long; whereas in their seasons of prosperity the reverse happens, the women wearing their hair long, and the men close, as stated by Plutarch. In the country now under consideration, the duration of these indications of mourning are variable, and are evidently guided more by the social position of the deceased, and the amount of wealth he has accumulated, than other motives. For the poorer class of natives and others of limited means, the prescribed probation is about six months; to caboceers and other personages of note, one year; while the mulatto grandees, from their assumption of superiority, exact the dedication of two years and upwards to their memory. Upon the notification of a death to the inhabitants of the town, the relatives, family connections, and other intimate friends, assemble together for the object of establishing a custom or feast in honour of the departed, the representation of which would rather suggest to the stranger, on first sight, that he was witnessing some popular exhibition of conviviality, than the preliminary scene of lamentation and woe. On the day preceding the interment, the populace generally congregate around the mansion of the deceased (where the corpse, elaborately adorned in all its paraphernalia of decoration, is exposed to view), and these fire off a number of muskets; dances and other fantastic evolutions subsequently occur, amid a concert of tomtoms and drums, that lend their aid to enliven the spectators. On such celebrations, great quantities of rum and other ardent liquors are quickly consumed, and intoxication is the usual result, which, if the interpretation of the natives be adopted, is solely induced with the laudable intention of dispelling the sorrow they then experience for the loss of their fellow-citizen. Upon the expiration of three weeks, another display of these ceremonies takes place, accompanied by the same peculiar exhibitions, after the cessation of which all further manifestations of respect on the part of his family and friends terminate, the requisite term of public mourning having been formally completed. According to the ancient laws of the country, the wives and other female relatives, particularly the former, are imperatively bound, at the finish of their allotted commemoration, to institute a corresponding custom, of a greater or less duration, proportionate to the extent of their resources. These rites ostensibly appear to have been established for the purpose of religiously enforcing the observance of those obligations due to the memory of the dead, to denote the dissolution of all prior ties or alliances, and also to shew that the females are at liberty to form new engagements (unless claimed by the succeeding family heritor), or enter into other matrimonial schemes as they find most conducive to their interest. With reference to the men, a compliance with these practices is less strictly exacted, and, therefore, such are not often prolonged beyond the brief interval of a few weeks or months as the case may be.

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Pawns, or other individuals who die heavily in debt, are denied the rights of sepulture; and unless some previous arrangement has been made with the creditor, are exposed on an elevated platform on the outskirts of the town, enshrouded by mats or enclosed in boxes, since the interment of the corpse would render his family liable for the payment of those bonds which the deceased had contracted when living. A similar interdict is said to exist in Kumassé and other Ashanté towns.

It is a somewhat singular fact, that among the ancient Egyptians an edict almost identical constituted one of the fundamental clauses promulgated in their judicial code for the regulation of commercial affairs. Herodotus asserts, that it was first enacted by Asychis, a king who merited the eulogium of being an illustrious benefactor to his subjects. The historian remarks that, in his reign "when commerce was checked and injured from the extreme want of money, an ordinance passed, that any one might borrow money, giving the body of his father as a pledge. By this law the sepulchre of the debtor became in the power of the creditor; for if the debt was not discharged, he would neither be buried with his family nor in any other vault, nor was he suffered to inter one of his descendants." *

Associated with other peculiar traits of much greater importance in former periods than at present, must be mentioned the strange decree, which, grounded on the faith of their primitive traditions, and the superstitious dread of witchcraft, compels the exhumation of the bodies of those people who have been suspected of being too intimately concerned with the supernatural influences during their lifetime. Natives who have been prematurely cut off, either from the inroads of some occasional epidemic, or the ordinary maladies of the season, are frequently supposed to become endowed with the potent prerogative of generating disease and destroying life; hence it is not an uncommon occurrence, when two or three members of the same family die in succession, to attribute their departure to the agency of the first sufferer or sufferers, the corpses of which, after satisfactory evidence has been adduced, are summarily removed from their houses within the sanctuary of which they had been interred, are ignominiously burned on the outskirts of the town, and their ashes scattered to the winds, amid the mingled groans and execrations of the populace. It matters not how innocent the unfortunate persons might have been, nor yet how long they may have slept in the calm tranquillity of the grave. The voice of public opinion is unanimous; they are branded with the stigma of posthumous murderers, and the violation of those hallowed repositories in which they rest is imperiously demanded, and the destruction of their frail

* Lib. 2, c. 136.

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contents accomplished without either dread or compunction. That which, under other circumstances, would be estimated as a crime of no trivial magnitude, is now proclaimed to be a meritorious deed, by the delay or non-performance of which the safety and welfare of the whole community are compromised.

Upon the event of the death of any individual in a distant country, though years should have elapsed since its annunciation had transpired, the relatives and connections, when a fit opportunity presents itself, despatch a party in search of the place of interment, and they, gathering together the mouldering remnants of mortality, return to bury them under the same roof as those of his ancestors. This custom,—which appears to resemble a labour of fidelity due to the memory of the deceased that his bones should not lie among those of strangers, but be blended with those of his family and kindred, so that the cherished remembrances and associations engendered in the past, should not be dissolved in the world to come, —has possibly originated from some of those primitive sanatory mandates which restricted the burial of the dead within definite bounds, or in pursuance to family compacts that exacted a compliance with certain intramural regulations of immemorial usance.

Inheritances, &c.—The law of inheritance, a conspicuous feature in the social institutions of many nations of Western Africa, must be distinguished as the grand pervading principle on which are based the disposition of property and power. This law can only be appreciated from the fact, that the consolidation or dispersion of family influence, the position and stability of subordinate branches, with the control of other kindred interests, are chiefly governed by the absolute right of a well-defined grade of relationship, exclusively derived through the blood on the female side. Divers reasons have been assigned for their advocacy of this genealogical system; but those hitherto brought forward have not proved sufficiently explanatory. No traces respecting its date of adoption or traditional introduction can be ascertained; for all that is known in connection with the subject may be comprised in the brief reply, that their ancestors transferred it from father to son, from such an early age that its source has long been lost in the mists of antiquity. Probably, among the more feasible arguments advanced in support of its tolerance, is that which refers to the woman the peculiar privilege of transmitting the family blood in a less uncertain stream from one person to another; so that, in its descent, it never could be entirely eradicated by an admixture with that from other channels; for, whatever marriages might be contracted by the mother or her female descendants, even from one generation to another in a continuous series, still there would always remain a sufficiency to ensure the original characteristics of her progenitors from being destroyed. Again, in further confirmation of these views, it has

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been asserted, that should the wife be guilty of any criminal intercourse with other parties, the same observations would apply with equal precision, inasmuch as the offspring must, at least on the maternal side, enjoy no inconsiderable portion of the ancestral lineage. With the husband or male, on the contrary, they remark, that after a few generations the blood becomes progressively diverted into other courses, and, proceeding downwards, is ultimately absorbed into the families of those females to which they have been allied.

It is the scrupulous regard paid to these fundamental distinctions of consanguinity, that also determines the choice and elevation of the royal aspirants who may be called to occupy the Ashanté throne.

Upon the death of any individual, his property invariably descends to his brothers and sisters in direct rotation, and not to his issue, as is the custom in more civilized communities. Should no brothers exist, the eldest sister succeeds in full, and subsequently her children, notwithstanding all her predecessors may have left large progenies behind them. It may therefore be considered as a general axiom, that the son seldom, if ever, inherits the estate of his father, which, from a deficiency in the proper collateral kindred, passes to the nephew or niece by the next sister's side. The only deviations from this rule are when the man has no other heir by any of his female relatives; under these circumstances, the first-born male not only comes into possession of his father's but even his uncle's wealth, should their decease have preceded that of his parent. Of course it is clearly understood that the son always obtains the effects and valuables of his mother. But when an inhabitant dies without relatives to demand his inheritance, the oldest slave is commonly selected as the representative to supply this void. Several perceptible modifications of this law have, however, been effected within a few recent years, in consequence of the promiscuous alliances of Europeans and their descendants with the aboriginal women. These rules of inheritance will perhaps not inaptly be explained by the following illustration:—Should a freeman, for instance, have sexual communion with a female slave, and conception take place, the fruit of it is born in bondage, and, like its mother, is the property of her owner; but should a similar result follow where the father is a slave and the woman free, the offspring belongs to the mother, and, like herself, is equally free, since it partakes of the same recognised condition, and is endowed with full rights and immunities as if its birth had occurred under the most benignant auspices.

After the interment of the corpse, the next of kin, in the gradation previously assigned, assumes the guardianship of the family interests, by virtue of which he not only acquires the patrimony of the defunct, but an undisputed right over his wives, children, and slaves, the former being for the most part superadded to his own establishment if the heritor be a man, while the two latter become incorpo-

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rated as component portions of his household. Over the sons and daughters, therefore, he is supposed to exercise all the functions and prerogatives of a parent, and in this capacity to administer to their wants, superintend their conduct, and determine their future settlement in life; and they in return are bound to yield him the full extent of their services, and to pay him that amount of submission, deference, and respect which is due to the position he fills. As their support and maintenance are solely derivable from the relative in charge, if it may be so expressed, during the period of their servitude, and implicit obedience required in exchange, it necessarily ensues that their treatment is in a great measure guided by the degree of subserviency rendered; so that, in fact, until their arrival at the age of maturity, they gradually degenerate into mere dependents upon his bounty, and are compelled, in compliance with his mandates, to perform such menial and other debasing avocations as he may choose to delegate to them.

Division of Time, &c.—In the computation of time they rarely adhere to the systems of more enlightened nations, by the subdivision of the year into a given number of moons or months, but rather prefer the adoption of a more primitive formula, derived from the observance of various climatic changes, the rotation of seasons, and other physical phenomena, and it is chiefly by such simple means that not only these but other tribes in Western Africa, are influenced in the regulation of their year; and it is this distribution alone that constitutes the fundamental principles on which these peculiar arrangements are based. Conformably to the established usages of each country, deviations and distinctions in their primary division are of common occurrence, and such variations are to be attributed more to a relaxed or stricter classification of climatorial agencies than from any artificial distinctions suggested by themselves, since an analytic examination into their respective merits would unquestionably point out that the majority, if not the whole, come under one prescriptive rule of formation, and proceed from the same definite basis as those in general prevalence throughout other countries on the African continent. In Akkrah, and the circumjacent districts, the year has been partitioned into three grand seasons, referable to the preceding mode; and these again, in some localities, seem to have been divided into still minor fractions. As considerable doubt has been expressed in relation to the latter, it is unnecessary for me to dilate further upon the subject. The designations of the primary seasons are thus annexed:

Summer.	Boo'ornah.	Mar. April, May, June, July, Aug.
Second Summer.	G'boh.	September, October, and November.
Winter.	Arrab-attah, said to be derived from the word Harmattan.	December, January, and February.

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The week consists of seven days, which are separately distinguished by appropriate cognomens, apparently corresponding to the number of days comprehended in the European calendars, and which may also be rendered as follows :—

Sunday	Haughbah.
Monday	Dhu.
Tuesday	Dhu-foh.
Wednesday	Shau.
Thursday	So.
Friday	So-ah.
Saturday	Hau.

Two of these days may be considered as sacred, viz. :—Dhu-foh, dedicated to the propitiation of *Ni* and the *River Sakkoom*, the great national fetish of Akkrah ; and Haughbah, devoted to the mysterious rites of *Oëyardo*, the dreaded patroness of all married women. It is a remarkable fact, when taken in connection with their religious duties, that, on the first of these days (Dhu-foh), no fisherman dare venture to launch his canoe upon the ocean's surface to gain his precarious livelihood, but guardedly abstains from those piscatory pursuits which might betray him or his family into the infringement of the superstitious mandates so solemnly enunciated by the priests and fetishmen. Similar stringent precautions are equally enjoined on the second (Haughbah) ; and though of a somewhat different character, are made compulsory on all ranks and sexes, but more exclusively to that of the female. Under the supposition that some malign potency pervades the surrounding country on this day, more particularly directed against the pregnant women, their daily avocations are restricted within the walls of their domiciles, no egress being tolerated either for the purposes of travelling or other exterior occupations. Not many people therefore presume to violate these injunctions by issuing forth early in the forenoon, and none resort to their familiar haunts in the markets or public thoroughfares, until the prohibition has been withdrawn by the well-known sign of a declining sun. In some respects So-ah may likewise be appended to the two previous days, owing to its being consecrated to *Kaulé* or the salt-pond fetish, which is one held in much less estimation, and therefore, is not entitled to the same amount of deference or veneration awarded to the others. The celebration of these religious obligations differ more or less as to their day of fulfilment in the various towns where such traditional forms of worship are systematically maintained.

Currency, &c.—The currency of the Gold Coast is represented by the Indian cowrie (*Cypræa moneta*) a small shell originally exported and carried from the east, and now diffused in vast quantities throughout the contiguous inland kingdoms and other central regions of

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Western Africa. For the convenience of transmission or payment, they were formerly perforated and strung together in definite numbers, hence the source of their designations into strings and heads. By a simple arrangement their fractional division was reduced to a standard, and found most beneficially adapted to the wants of the population. The annexed table will prove duly explanatory of their system.

Heads.	Strings.	Number of Cowries.	English value. £	s.	d.
...	½	20	0	0	0½
...	1	40	0	0	1
...	12	480	0	1	0
1	48	1,920	0	4	0
1 ounce of gold dust, or 20	960	38,400	4	0	0

The rate of exchange, when dollars require to be converted into cowries, and *vice versa*, will depend upon their current value at the different outports where the requisition is made. Thus, at Cape Coast, the dollar is estimated at 4s. 6d., in Akkrah at 5s. currency, and in other places along the coast, at its sterling price, 4s. 2d. The equivalents therefore to be given in cowries for each, should amount to the following.

Dollar at	5s.	=	60 strings	=	2400	cowries.
	4s. 6d.	=	54 do.	=	2160	do.
	4s. 2d.	=	50 do.	=	2000	do.

Gold dust, one of the staple articles of commerce exported from this tract of African coast, is more plentiful at the Fanté towns of Annamabo and Cape Coast, than in those of Akkrah. It is brought to the former places by the Akim and Ashanté traders from their own and the circumjacent countries, and has been considered by Adams and other European authorities to be much inferior in quality to that obtained from Apollonia and Dixcove. A great quantity was annually poured into Akkrah for a series of years previous to the present date; but this, from a multiplicity of causes, became gradually diminished, and was ultimately diverted into other channels. This diminution is to be ascribed to the Ashantés manifesting a preference for those markets in which were exhibited for choice a richer assortment of merchandise, better suited to their demands, and from the fact that to the eastward of Christiansburg little or no gold could be purchased, owing to the soil being less fertile in those auriferous depositions than the surface of various localities in the inland and maritime provinces of the west. The gold offered for sale or barter, is ordinarily adulterated according to the ingenuity of the vendor or inexperience of the buyer. These adulterations comprise copper or brass filings, pieces of impure ore, micaceous earths, and granular alloys of silver, and other analogous substances calculated to deceive

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the eye. They, however, are detected without trouble or difficulty, under the customary supervision of a native personage, professionally denominated a gold-taker, whose services are specially retained in mercantile establishments for this object. European factors regulate their purchases and computations by its artificial division into ackies and ounces, of which sixteen of the former, valued respectively at five shillings, constitutes the ounce, that again being equal to four pounds currency. The country people in their multifarious trading speculations, are subject to a constant fluctuation of prices produced by the interchange of commodities among the various tribes with whom they come in contact. As these comprehend individuals in every sphere of life, an enlargement of the scale of equivalents in gold dust became requisite, and has been fully accomplished by the Fanté and and Ashanté traders, by their minute subdivision and combination of the ounce into a minor variety of terms, each of which has its relative value affixed. This pecuniary method of valuation so far suffices for the mutual accommodation of all parties engaged in traffic, and has implicitly guided hitherto the inhabitants of Akkrah, Adampé, and the more eastern nations in their commercial transactions. It is somewhat remarkable, that the native appellation of *Seekah* is known not only throughout every portion of the Gold Coast, but in Popo, Dahomey, and the distant regions of Yorruba. The following tables of the gold currency of the Fanté and Ashanté nations compiled by the late Mr M'Lean, are equally adopted by the people of Akkrah and Adampé, and the accuracy of which may be sufficiently guaranteed by the well-known experience of their author.

TABLE I.—*Fanté Currency.*

Names of Weights.	Weight in oz.	Weight in ackies.	Value. £	s.	d.	Names of Weights.	Weight in oz.	Weight in ackies.	Value. £	s.	d.
Pessua	...	1-48th	0	0	1½	Essien	...	6	1	10	0
Simpoah	...	1-24th	0	0	2½	Acandjua . . .	...	7	1	15	0
Takufan	...	1-12th	0	0	5	Djua	½	8	2	0	0
Kokua	...	1-8th	0	0	7½	Sul	...	9	2	5	0
Taku	...	1-6th	0	0	10	Sua-ne-sul . . .	...	13½	3	7	6
Suafan	...	5-6ths	0	4	2	Djuamien . . .	1	...	4	0	0
Meaton (*or Giri fan*)	...	1	0	5	0	Essuanu	1	2	4	10	0
Sua	...	1-4-6th	0	8	4	Djuamiensan . .	1	8	6	0	0
Agiratjwi (*or Gira*)	...	2	0	10	0	Essuasan . . .	1	11	6	15	0
Ensan	...	3	0	15	0	Bendah	2	...	8	0	0
Djuasul	...	4	1	0	0	Perigwan . . .	2	4	9	0	0
Perisul	...	5	1	5	0	Entenu	4	8	18	0	0

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TABLE II.—*Ashanté Currency.*

Names of Weights.	Weight in oz.	Weight in ackies.	Value. £	s.	d.	Names of Weights.	Weight in oz.	Weight in ackies.	Value. £	s.	d.
Pessua	...	1-64th	0	0	0½	Insuansan	...	2⅛	0	11	8
Damba	...	1-32d	0	0	1⅛	Bodomu	...	2½	0	12	6
Takufan	...	1-16th	0	0	3⅛	Ensan	...	3	0	15	0
Taku	...	1-8th	0	0	7½	Djuasul	...	3½	0	17	6
Taku-mienu	...	1-4th	0	1	3	Sul	...	4½	1	2	6
Takumiensan	...	3-8ths	0	1	10½	Perisul	...	5	1	5	0
Suafan	...	3-4ths	0	3	9	Essien	...	6	1	10	0
Dumafan	...	11-12ths	0	4	7	Djua	...	7	1	15	0
Brofan	...	1	0	5	0	Anenfii	...	7⅛	1	19	2
Agiratjwifan	...	1 1/12th	0	5	5	Esua	...	9	2	5	0
Insuansafan	...	1⅛th	0	5	10	Suane-sul	...	13½	3	7	6
Bodombufan	...	1½d	0	6	8	Essua-nu	1	2	4	10	0
Sua	...	1⅝d	0	7	6	Essua-san	1	11	6	15	0
Duma	...	1⅞th	0	9	2	Essua-san-sul	2	...	8	0	0
Brofu	...	2	0	10	0	Perigwan	2	4	9	0	0
Agiratjwi	...	2⅛th	0	10	10	Entenu	4	8	18	0	0

N.B.—An ackie is equal to 8 Ashanté takus, and to 6 Fanté takus.

(*To be concluded in our next Number.*)

On the supposed analogy between the Life of an Individual and the Duration of a Species. By EDWARD FORBES, Esq., F.R.S., &c. Communicated by the Author.*

In Natural History and Geology a clear understanding of the relations of Individual, Species, and Genus to Geological Time and Geographical Space is of essential importance. Much, however, of what is generally received concerning these relations will scarcely bear close investigation. Among questionable, though popular notions upon this subject the lecturer would place the belief that the term of duration of a species is comparable and of the same kind with that of the life of an individual.

The successive phases in the complete existence of an individual are, Birth, Youth, Maturity, Decline, and Decay, terminating in Death. Whether we regard an individual as

* Read before the Royal Institution of Great Britain on 7th May 1852.

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a single self-existing organism, however produced, or extend it to the series of organisms, combined or independent, all being products of a single ovum, its term of duration can be abbreviated but not prolonged indefinitely, nor can the several phases of its existence be repeated. Conditions may arrest or hasten maturity, or prematurely destroy, but cannot, however favourable, reproduce a second maturity after decline has commenced.

Now, it is believed by many that a species (using the term in the sense of an assemblage of individuals presenting certain constant characters in common, and derived from one original protoplast or stock) passes through a series of phases comparable with those which succeed each other in definite order during the life of a single individual,—that it has its epochs of origin, of maturity, of decline, and of extinction, dependent upon the laws of an inherent vitality.

If this notion be true, the theory of Geology will be proportionately affected; since in this case the duration of species must be regarded as only influenced, not determined, by the physical conditions among which they are placed;—and, thus, species should characterise epochs or sections of time, independent of all physical changes and modifying influences short of those which are absolutely destructive. Now, geological epochs, as at present understood, are defined by peculiar assemblages of species, and the amount of change in the organic contents of proximate formations or strata is usually accepted as a measure of the extent of the disturbances that affect them. Yet this latter inference, involving as it does the supposition that the spread and continuity of species in time is dependent upon physical influences, is adverse to the notion of a Life of a Species, as stated above.

If we seek for the origin of this notion we shall find that is has two sources, the one direct, the other indirect. It is not an induction, nor pretended to be, but an hypothesis assumed through apparent analogies. Its first and principal source may be discovered in the comparison suggested by certain necessary phases in the duration of the species with others in the life of an individual, such as, each has its com-

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mencement, and each has its cessation. Geological research has made known to us that prior to certain points in time certain species did not exist, and that after certain points in time certain species ceased to be. The commencement of a species has been compared with *Birth*, the extinction with *Death*. Again, many species can be shewn to have had an epoch of maximum development in time. This has been compared with the maturity of the individual.

Between the birth of an individual and the commencement of a species in the first appearance of its protoplast, the analogy is more apparent than real. We know how the former phenomenon takes place, but we have no knowledge of the latter.

Between the maturity of the individual and the maximum development of a species there is no true analogy, since the latter can easily be proved to be entirely dependent on the combination of favouring conditions, and during the period of duration of a species there may be two or more epochs of great or even equal development, and two or more epochs of decline alternating with epochs of prosperity. The epoch of maximum of a species may also occur during any period in its history short of the first stage. Geological and geographical research equally shew that the flourishing of a species is invariably coincident with the presence of favouring, and its decline with that of unfavourable conditions. Hence there is no analogy between the single and definite phase of maturity of the individual and the variable and sometimes often repeated epochs of luxuriant development in the duration of a species.

Between the death of the individual and the extinction of a species there is an analogy only when the former event occurs prematurely, through the influence of destroying conditions. But in their absence, an individual after its period of vitality has been completed must necessarily die ; whereas we have no right to assume that such would be the fate of a species so circumstanced, since in every case where we can, either geologically or geographically, trace a species to its local or general extinction, we can connect the fact of its disappearance with the evidences of physical changes.

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[The lecturer illustrated these points by diagrams and special demonstrations, selecting for explanation two local cases, the one marine and the other fresh water; the former taken from the geological phenomena of Culver cliff and the neighbouring bays in the Isle of Wight, of which a beautiful and original model had been communicated by Captain Ibbetson for the purpose; and the latter from his own recent researches (unpublished) on the succession of organic remains in the Purbeck strata of Dorsetshire, conducted as part of the labours of the Geological Survey of Great Britain.]

The second and more indirect source of the notion of *the life of a species* may be traced in apparent analogies, half-perceived, between the centralisation of generic groups in time and space, and the limited duration of both *species* and *individual*. But in this case ideas are compared which are altogether and essentially distinct.

The nature of this distinction is expressed among the following propositions, in which an attempt is made to contrast the respective relations of *individual*, *species*, and *genus* to Geological time and Geographical space.

A. The *individual*, whether we restrict the word to the single organism, however produced—or extend it to the series of organisms, combined or independent, all being products of a single ovum—has but a limited and unique existence in time, which, short as it must be, can be shortened by the influence of unfavourable conditions, but which no combination of favouring circumstances can prolong beyond the term of life allotted to it according to its kind.

B. The *species*, whether we restrict the term to assemblages of individuals resembling each other in certain constant characters, or hold, in addition, the hypothesis (warranted, as might be shewn from experience and experiment), that between all the members of such an assemblage there is the relationship of family, the relationship of descent, and consequently that they are all the descendents of one first stock or protoplast—(how that protoplast appeared is not part of the question)—is like the individual, in so much as its relations to *time* are *unique:* once destroyed, it never reappears.

But (and this is the point of the view now advocated),

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unlike the *individual*, it is continued indefinitely so long as conditions favourable to its diffusion and prosperity—that is to say, *so long as conditions favourable to the production and sustenance of the individual representatives or elements are continued coincidently with its existence.*

[No amount of favouring conditions can recal a species once destroyed. On this conclusion, founded upon all facts hitherto observed in palæontology, the value of the application of Natural History to Geological science mainly depends.]

C. The *genus*, in whatever degree of extension we use the term, so long as we apply it to an assemblage of species intimately related to each other in common and important features of organisation, appears distinctly to exhibit the phenomenon of centralization in both *time* and *space*, though with a difference, since it would seem that each *genus* has a *unique centre* or *area of development* in time, but in geographical space may present *more centres than one.*

a. An individual is a positive reality.

b. A species is a relative reality.

c. A genus is an abstraction—an idea—but an idea impressed on nature and not arbitrarily dependent on man's conceptions.

α. An individual is *one.*

β. A species consists of *many resulting from one.*

γ. A genus consists of more or fewer of the *manies resulting from one* linked together not *by a relationship of descent*, but *by an affinity dependent on a Divine idea.*

a. An individual cannot manifest itself in two places at once; it has no extension in space; its relations are entirely with *time*, but the possible duration of its existence is regulated by the law of its inherent vitality.

b. A species has correspondent and exactly analogous relations with time and space—the duration of its existence as well as its geographical extension is entirely regulated by physical conditions.

c. A genus has dissimilar or only partially comparable relations with time and space, and occupies areas in both, having only partial relations to physical conditions.

The investigations of these distinctions and relations form

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the subject of a great chapter in the philosophy of Natural History. That philosophy contemplates the laws that regulate the manifestation of life exhibited in organised nature, and their dependence upon and connection with the inorganic world and its phenomena. None teaches more emphatically the difficulties with which man's mind must contend when attempting to comprehend the wisdom embodied in the universe, and none holds out a more cheering prospect of future discovery in fresh and unexpected fields of delightful research.

Lectures on the Results of the Great Exhibition of 1851, *delivered before the Society of Arts, Manufactures, and Commerce, at the suggestion of His Royal Highness Prince Albert, President of the Society.**

The following are the subjects discussed in these Lectures:

I. The General Bearing of the Great Exhibition on the Progress of Art and Science. By the Rev. W. Whewell, D.D., F.R.S., Master of Trinity College, Cambridge.

II. Mining, Quarrying, and Metallurgical Processes and Products. By Sir H. T. De la Beche, C.B., F.R.S.

III. The Raw Materials from the Animal Kingdom. By Richard Owen, F.R.S.

IV. Chemical and Pharmaceutical Processes and Products. By Jacob Bell, Esq., M.P.

V. The Chemical Principles involved in the Manufactures of the Exhibition, as indicating the Necessity of Industrial Instruction. By Lyon Playfair, C.B., F.R.S.

VI. Substances used as Food, illustrated by the Great Exhibition. By John Lindley, Ph.D., F.R.S., Professor of Botany in University College, London.

VII. The Vegetable Substances used in the Arts and Manufactures, in relation to Commerce generally. By Professor Edward Solly, F.R.S.

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VIII. Machines and Tools for Working in Metal, Wood, and other Materials. By the Rev. Robert Willis, M.A., F.R.S., Jacksonian Professor in the University of Cambridge.

IX. Philosophical Instruments and Processes, as represented in the Great Exhibition. By James Glaisher, Esq., F.R.S.

X. Civil Engineering and Machinery generally. By Henry Hensman, Esq.

XI. The Arts and Manufactures of India. By Professor J. F. Royle, M.D., F.R.S.

XII. On the Progress of Naval Architecture, as indicating the Necessity for Scientific Education, and for the Classification of Ships and Steam-Engines; also on Life-Boats. By Captain Washington, R.N., F.R.S.

Of these interesting lectures, the first or leading, viz. the admirable discourse of Dr Whewell, has already appeared in this Journal (*vide* Vol. lii. No. 103, January 1852, p. 1). It would have afforded us much pleasure to have gone fully into the merits of the other lectures, but our limits prevent this. The following extracts from some of these lectures will, however, we think, enable our readers to judge of the kind of information they afford.

I.—Sir Henry De la Beche.

1. *Amount of British Iron.*—The Exhibition may be said to have given rise to the most complete view of the iron produce of this country which we possess. Mr Samuel Blackwell, himself an ironmaster, accompanied the collection of iron ores by a statement of great value. He estimates the gross annual production of iron in Great Britain to be now upwards of 2,500,000 tons. Of this quantity, South Wales furnishes 700,000 tons; South Staffordshire (including Worcestershire), 600,000 tons: and Scotland 600,000 tons. The remainder is divided among the various smaller districts. The iron of England and Wales was produced by 336 furnaces in blast in 1850. Though a considerable quantity of British iron is exported, a very large proportion remains to be variously employed in our own industry.

2. *Desilverising of Lead.*—As to lead, the illustrations were chiefly British. There was an excellent exhibition of Pattinson's important process for desilverising that metal—a process

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which has been of such service to lead-mining generally, rendering many lead-mines workable with profit which must otherwise have been abandoned. The chief ore whence lead is extracted is that known as galena, or the sulphuret of lead, furnishing from seventy-five to eighty-three parts of the metal according to purity. It usually, though not always, contains silver in variable proportions. Upon the quantity of silver often depends the profitable raising of the ore. Previous to the invention of Mr Pattinson (of Newcastle-upon-Tyne), about twenty ounces of silver in the ton of lead were required to render the extraction of that metal worth the cost; since then as little as three and four ounces in the ton of lead will repay extraction. Now, as so many ores contain small quantities only of silver, the importance of the process is evident. In a scientific point of view it is one of much interest, as it consists in so conducting the work that portions of the lead can crystallise, by which the silver becomes excluded, in the manner in which, in many crystallising processes, foreign substances are excluded during crystallisation. Thus, by degrees, a mass of mixed lead and silver is left, extremely rich in the latter. When this richness in silver arrives at the point desired, that metal is extracted in the usual manner by cupellation. The lead-smelting at the Allenhead's mines, and at the Wanlockhead Hills, Dumfriesshire, both excellently displayed, are both founded on Pattinson's process. While touching on the Wanlockhead Hills exhibition, we should not pass over the arrangements by which the fumes from the furnace are prevented from escape, and from damage to the surrounding country, while lead, to the amount of thirty-three per cent. from the deposits or "fume" is obtained.

3. *Plumbago.*—The importance of plumbago for the arts and for crucibles is well known. After the Borrowdale mines, Cumberland, were somewhat exhausted, it became important, for that variety of plumbago employed in arts, to obtain some substitute; and varieties of compounds were invented, but nothing succeeded so well as the compressing process presented by Mr Brockedon, of which illustrations were in the Exhibition. By this process much of the Borrowdale plumbago dust has been utilised with advantage. It, or any other good plumbago, is ground into fine powder, placed in packets, and then receives a pressure equal to about 5000 tons. To prevent the injurious effect of disseminated air in the packets of fine powder, it is extracted by means of an air-pump, and thus the particles themselves can be brought into close juxtaposition and forced to cohere. Of the application of plumbago to crucibles there were several examples, some well known for their quality.

II. Professor Owen.

1. *Geology of the Sheep.*—The recent progress of palæontology,

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or the science of fossil organic remains, remarkable for its unprecedented rapidity, adds a new element to the elucidation of this question, which was so ably discussed by Buffon and the naturalists of the last century. At present, however, the evidence which palæontology yields is of the negative kind. No unequivocal fossil remains of the sheep have yet been found in the bone caves, the drift, or the more tranquil stratified newer pliocene deposits, so associated with the fossil bones of oxen, wild boar, wolves, foxes, otters, beavers, &c., as to indicate the coevality of the sheep with those species, or in such an altered state as to indicate them to have been of equal antiquity. I have had my attention particularly directed to this point, in collecting evidence for a "History of our British Fossil Mammalia." Wherever the truly characteristic parts, viz., the bony cores of the horns, have been found associated with jaws, teeth, and other parts of the skeleton of a ruminant, corresponding in size and other characters with those of the goat and sheep, in the formations of the newer pliocene period, such supports of the horns have proved to be those of the goat.* No fossil horn-core of a sheep has yet been anywhere discovered; and so far as this negative evidence goes, we may infer that the sheep is not geologically more ancient than man; that it is not a native of Europe, but has been introduced by the tribes who carried hither the germs of civilisation in their migrations westward from Asia.

2. *Baleen.*—I have next to speak of a substance which, though commonly called "whalebone," has nothing of the nature of bone in it; but it is an albuminous tissue, nearly allied to hair and bristles, both in its chemical and vital properties, and its mode of development.

Of all the creatures which man has subdued for his advantage and use, that which surpasses every other animal in bulk, and which lives in an element unfitted for man's existence, might be supposed to be the last that he would have the audacity to attack, or the power to overcome. The great whales, that "tempest the ocean," are able, as many instances—and a very recent one—have shewn, to stave in the bottom of a ship by a blow of their muzzle, and crack a boat by a nip of their jaws, as easily as we would a nut—"Si sua robora norint!" If they did but know their strength, and how to use it, pursuit would be in vain, and whales would become the most dreaded, instead of the most coveted, of the denizens of the deep.

* A characteristic fossil of this kind, found associated with remains of the Mammoth and leptorine rhinoceros in the newer fresh-water pliocene of Walton, in Essex, is figured in my "History of British Fossil Mammalia," p. 489, cut 204.

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The cetaceans, which afford the whalebone, or, more properly, baleen plates, are of a more timid nature than the great sperm whales, which commonly cause the catastrophes alluded to: they have no teeth, but in their place they have substitutes, in the form of horny plates, ending in a fringe of bristles,—a peculiarity first pointed out by Aristotle.* Of these plates, properly called "baleen," the largest, which are of an equilateral triangular form, are arranged in a single longitudinal series on each side of the upper jaw, situated pretty close to each other, depending vertically from the jaw, with their flat surfaces looking backwards and forwards, and their unattached margins outwards and inwards, the direction of their interspaces being nearly transverse to the axis of the skull. The smaller subsidiary plates are arranged in oblique series, internal to the marginal ones. The base of each plate is hollow, and is fixed upon a pulp developed from a vascular gum, which is attached to a broad and shallow depression occupying the whole of the palatal surface of the maxillary and of the anterior part of the palatine bones. The base of each marginal plate is the smallest of the three sides of the triangle; it is unequally imbedded in a compact subelastic substance, which is so much deeper on the outer than on the inner side, as in the new-born whale to include more than one-half of the outer margin of the baleen plate. The form of the baleen-clad roof of the mouth is that of a transverse arch or vault, against which the convex *dorsum* of the thick and large tongue is applied when the mouth is closed. Each plate sends off from its inner and oblique margin the fringe of moderately stiff but flexible hairs which projects into the mouth. The bases of the baleen plates do not stand far apart from one another, but the anterior and posterior walls of the pulp fissure are respectively confluent with the contiguous divisions of the bases of the adjoining plates at their thin and extreme margins, which, by this confluence, close the basal end of the interspace of the baleen plates, which interspace is occupied more than half way down the plate by the cementing substance or gum. Thin layers of horn, in like manner, connect the contiguous plates, and may be traced, extending in parallel curves with the basal connecting layer, across the cementing substance.

The baleen pulp is situated in a cavity at the base of the plate, like the pulp of a tooth; whilst the external cementing material maintains, both with respect to this pulp, and to the portion of the baleen plate which it develops, the same relation as the dental capsule

* The passage occurs in the 12th chapter of the 3d book of the "Historia Animalium," and has given rise to much speculation and controversy:—"Mysticetus etiam pilos in ore intus habet vice dentium suillis setis similes." To a person looking into the mouth of a stranded whale, the concavity of the palate would appear to be beset with coarse hair. The species of *Balænoptera*, which frequents the Mediterranean, might have afforded to the father of zoology the subject of his comparison.

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bears to the tooth. According to these analogies, it must follow that the only central fibrous or tubular portion of the baleen plate is formed, like the dentine, by the basal pulp, and that the base of the plate is not only fixed in its place by the cementing substance or capsule, but must also receive an accession of horny material from it.

The baleen plates are smallest at the two extremities of the series; in the southern whale (*Balæna Australis*) they rapidly increase in length to the thirtieth, then very gradually increase in length to about the one hundred and fortieth; from this they as gradually diminish to the one hundred and sixtieth plate, and thence rapidly slope away to the same small size as that with which the series commenced. Besides the external, and, as they may be termed, the normal plates, which have just been described, there are developed from the inner part of the palatal gum, in the *Balæna Australis*, a series of smaller fringed processes, progressively decreasing in size as they recede from the large external plates; the small plates clothe the middle region of the palate with a finer kind of hair, against which the surface of the tongue more immediately rests; they are also arranged in longitudinal series, which, however, are not parallel with the external one, but pass from the inner margin of that series in oblique lines inwards and backwards.

In the great northern whale, (*Balæna mysticctus*), the balæn plates which succeed the large ones of the outer row are more numerous, and are relatively longer and larger than in the *Balæna Australis*. Mr Scoresby, who, in his account of the *Balæna mysticetus*, notices only the marginal plates, states that they are about two hundred in number on each side; the largest are from ten to fourteen feet, very rarely fifteen feet in length, and about a foot in breadth at their base. These plates are overlapped and concealed by the under lip when the mouth is shut. In the *Balænopteræ*, or fin-backed whales, the baleen processes, internal to the marginal plates, are fewer and smaller than in the true whales (*Balænæ*.) The marginal plates are more numerous, exceeding three hundred on each side; they are broader in proportion to their length, and much smaller in proportion to the entire animal; they are also more bent in the direction transverse to their long axis.

Each plate of the baleen consists of a central coarse fibrous substance, and an exterior compact fibrous layer; but this reaches to a certain extent only, beyond which the central part projects in the form of the fringe of bristles. The chemical basis of baleen, according to the experienced Professor Brande, is albumen, hardened by a small proportion of phosphate of lime.*

* "For the microscopical characters, and other particulars of the baleen plates, I must refer to my *Odontography*, vol. i., p. 311."

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The final purpose of this singular armature of the upper jaw of the great whales, is to secure the capture and retention of the small floating molluscs and crustaceans, which serve principally as their food. When the capacious mouth is opened, the water rushes in, and is strained through the fringed surface of the roof and sides, whilst the small animals are retained, bruised against the stiff bristled margins of the plates, and swallowed.

Baleen, or whalebone, from its tenacity, flexibility, elasticity, compactness, and lightness, is applied to a great variety of useful purposes. These were well exemplified in the collection exhibited under No. 103, by Mr Henry Horan, which shewed well-selected examples of whalebone plates from the Arctic whale (*Balæna mysticetus*), which yields the largest and best kind; from the Antarctic whale (*Balæna Australis*), which affords the second best kind; and from the great finner whale (*Balænoptera hoops*), which affords the shortest and coarsest plates. With these examples of the raw material, Mr Horan exhibited specimens of the raw material in various states of preparation, and numerous and ingenious applications of the prepared baleen, dyed of different colours, as, *e. g.*, for covering whip-handles, walking-sticks, and telescopes, and in the form of shavings for plaiting, like straws, in the construction of light hats and bonnets. An excellent and instructive series of preparations of baleen was also exhibited by Messrs Westall, in which was more especially deserving notice the great variety of filamentary modifications of the whalebone material for numerous useful applications. Fine blades of whalebone from the *Balæna mysticetus* were exhibited in the United States department, under No. 531, by Mr L. Goddard, and characteristic specimens of baleen plates from the *Balæna Australis* had been transmitted by Mr G. Moses from Van Diemen's Land.

3. *Ivory.*—The same considerations necessarily limited the functions of our jury, in regard to the tusks of animals presenting the modification of dental substances to which the term "ivory" is applied. Fine ivory, distinguished by the decussating curved lines on the surfaces of transverse fractions or sections of the tusk, is peculiar to the African and Asiatic elephants, amongst existing quadrupeds; and the best is obtained from the wild individuals; domestication of the elephant, in India at least, having been attended usually by deterioration of the length and quality of the tusks.

The finest specimens of elephant tusks sent to the Great Exhibition were a pair weighing 325 pounds, from the *Elephas Africanus*, obtained from an animal killed near the newly-discovered Lake Ngami, in South Africa. Each tusk measured 8 feet 6 inches in length, and 22 inches in basal circumference. A single tusk weighing 110 pounds, from the same locality, was

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associated with them. These specimens were exhibited by Mr Joseph Cawwood.

Messrs Fauntleroy and Sons exhibited an instructive collection of elephants' tusks in No. 135. The largest of these was also from the African elephant, and weighed 139 pounds. Varieties of tusks were exhibited from the Gold Coast, the Gaboon River, Zanzebar, the Cape of Good Hope, Angola, Alexandria, Ceylon, and the East Indies. Of the tusks which possess a dense texture, but have not the engine-turn markings of true ivory, Messrs Fauntleroy exhibit those of the narwhal, the walrus, and the hippopotamus; and the Jury regarded this instructive collection as deserving Honourable Mention.

Fine tusks of the Ceylon variety of elephants were shewn in the collection from that island; and several examples of the continental Asiatic kinds were exhibited in the Indian departments. Amongst the tusks of the Siamese elephants was one which weighed 100 pounds, and shewed a fine white compact kind of ivory.

4. *Feathers and Down.*—The most beautiful, the most complex, and the most highly elaborated of all the coverings of animals, due to developments of the epidermal system, is the plumage of birds. Well might the eloquent Paley say—"Every feather is a mechanical wonder. Their disposition—all inclined backward; the down about the stem; the overlapping of their tips; their different configuration in different parts; not to mention the variety of their colours—constitute a vestment for the body so beautiful, and so appropriate to the life which the animal has to lead, as that, I think, we should have had no conception of anything equally perfect, if we had never seen it, or can now imagine anything more so."

A feather consists of the "quill," the "shaft," and the "vane." The vane consists of "barbs" and "barbules."

The *quill* is pierced by a lower and an upper orifice, and contains a series of light, dry, conical capsules, fitted one upon another, and united together by a central pedicle.

The *shaft* is slightly bent; the concave side is divided into two surfaces by a middle longitudinal line continued from the upper orifice of the quill; the convex side is smooth. Both sides are covered with a horny material, similar to that of the quill; and they enclose a peculiar white, soft, elastic substance, called the "pith."

The *barbs* are attached to the sides of the shaft, and consist of plates, arranged with their flat sides towards each other, and their margins in the direction of the convex and concave sides of the feather; consequently they present considerable resistance to being bent out of their plane, although readily yielding to any force acting upon them in the direction of the line of the stem.

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The *barbules* are given off from either side of the barbs, and are sometimes similarly barbed themselves, as may be seen in the barbules of the long feathers of the peacock's tail.

The barbules are commonly short and close set, and curved in contrary directions; so that two adjoining series of barbules interlock together, and form the mechanism by which the barbs are compacted into the close and resisting vane of the quill, or "feather," properly so called. When the barbules are long and loose, they characterise that form of the feather which is properly called a "plume;" and such are the most valuable products of the plumage of birds in a commercial point of view; as, for example, the plumes of the ostrich.

The lower barbs in every kind of feather are usually loose, forming the down, which is increased, in most birds, by what is called the "accessory plume." This is usually a small downy tuft, but varies in different species, and even in the feathers of different parts of the body of the same bird. The value of feathers, for bed-stuffing, depends upon the proportion of loose soft down that enters into their composition; and as the "accessory plume" in the body-feathers of the swan, goose, and duck, is almost as long as the feather from which it springs, hence arises the commercial value of the feathers of these aquatic birds.

In the development of plumage, the first covering of the bird is a temporary one, consisting of bundles of long loosely-barbed filaments, which diverge from a small quill, and on their first appearance are enveloped in a thin sheath, which soon crumbles away after being exposed to the atmosphere.* These down feathers are succeeded by the true feathers; to which they bear the same relation as wool does to hair, or the temporary to the permanent teeth. In most birds, a certain proportion of the down feathers is retained with the true feathers, and this proportion is usually greatest in aquatic birds. It is most remarkable in the Eider Duck (*Anas mollissima*), which may be compared with the sheep in regard to the quantity and quality of the softer and warmer kind of the epidermal covering. The down of the eider combines with its peculiar softness, fineness, and lightness, so great a degree of elasticity, that the quantity of this beautiful material which might be compressed and concealed between the two hands of a man, will serve to stuff the coverlet of a bed.

All the varieties and modifications of the plumage of birds, serviceable in manufactures, or valued as ornaments, might be compared and studied with advantage in the Great Exhibition.

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* A good account of the mode of formation of feathers is given in a paper by M. F. Cuvier, entitled "Sur le developpement des Plumes," in the "Memoires du Museum," tom. x. 10; or the article "Aves," in the "Cyclopedia of Anatomy," may be consulted.

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An instructive and comprehensive collection of feathers and down, in different states of preparation for bed-stuffing, including English goose feathers, Irish goose and mixed feathers, Dantzic feathers, Russian goose feathers, and mixed duck feathers, Hudson's Bay goose and duck feathers, Russian down and Greenland eider down, were exhibited by Messrs Heal & Son. Messrs W. & C. Nightingale likewise exhibited an illustrative collection of feathers and down, shewing the effects of their mode of purifying feathers by steam, without the use of sulphurous gas.

In the Indian department were shewn white and black ostrich plumes; but these had been imported from Aden. If the ostrich ever steps into Asia, it is only a little way into the Arabian side of the Isthmus of Suez: the *Struthio camelus* belongs to a peculiarly African genus of the great wingless birds. Tippets, victorines, and boas made from the down of the young adjutant-crane (*Ciconia argala*) were exhibited from Commercolly; and also beautiful white feathers of a smaller species of crane from Arrahan.

5. *General Remarks on Materials from the Vegetable and Animal Kingdom.*—Whatever the animal can afford for food or clothing, for our tools, weapons, or ornaments—whatever the lower creation can contribute to our wants, our comforts, our passions, or our pride, that we sternly exact and take, at all cost to the producers. No creature is too bulky or formidable for man's destructive energies—none too minute and insignificant for his keen detection and skill of capture. It was ordained from the beginning that we should be masters and subduers of all inferior animals. Let us remember, however, that we ourselves, like the creatures we slay, subjugate, and modify, are the results of the same Almighty creative will—temporary sojourners here, and co-tenants with the worm and the whale of one small planet. In the exercise, therefore, of those superior powers that have been intrusted to us, let us ever bear in mind that our responsibilities are heightened in proportion.

III.—Dr Lyon Playfair.

1. *Iron-Smelting.*—Let us select the smelting of iron,* as an example of the teachings of chemistry. If practice, unaided by science, be sufficient for the prosecution of manufactures, this venerable art must be thoroughly matured, and science could scarcely expect to

* Although the smelting of iron is not strictly within the division of Manufactures, according to the classification, its importance to this country will authorise an exception in its favour.

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be of much use to it in its present state. But while we find much to admire in the triumphs of practical experience, there is yet great room for the improvement of this art. The cheapness of iron ore, and of the coal used in its smelting, has been so great that, regardless of their capital importance to this country, we, like careless spendthrifts, use them without thought of the future.

The mode of smelting iron consists in mixing the ore with lime and coal; the former producing a slag or glass with the impurities of the ore, while the coal reduces the oxide of iron to its metallic state. Much heat is required in the process of smelting, but the cold air blown in, as the blast, lowers the temperature, and compels the addition of fuel, as a compensation for this reduction. Science pointed to this loss, and now the air is heated before being introduced to the furnace. The quantity of coal is wonderfully economized by this application of science; for instead of seven tons of coal per ton of iron, three tons now suffice, and the amount produced in the same time is nearly sixty per cent. Assuredly this was a great step in advance. Could science do more?

Professor Bünsen, in an inquiry in which I was glad to afford him aid, has shewn that she can. We examined the furnaces, in each portion of the burning mass, so as fully to expose the operations in every part of the blazing structure. This seemingly impossible dissection was accomplished by the simplest means: the furnaces are charged from the top, and the materials gradually descend to the bottom; with the upper charge a long graduated tube was allowed to descend, and the gases streaming from ascertained depths were collected and analysed. Their composition betrayed with perfect accuracy the nature of the actions at each portion of the furnace, and the astonishing fact was elicited, that, in spite of the saving produced by the introduction of the hot blast, no less than 81½ per cent. of fuel is actually lost, only 18½ per cent. being realised. If, in round numbers, we suppose that four-fifths of the fuel be thus wasted, no less than 5,400,000 tons are every year thrown uselessly into the atmosphere; this being nearly one-seventh of the whole coal annually raised in the United Kingdom. This enormous amount of fuel escapes in the form of combustible gases, capable of being collected and economised; yet in spite of these well-ascertained facts, there are scarcely half-a-dozen furnaces in the United Kingdom where this economy is realised by the utilization of the waste gases of the furnace.

Large quantities of ammonia are annually lost in iron smelting, which might readily be collected. Ammonia is constantly increasing in value, and each furnace produces and wastes at least 1 cwt. of its principal salt daily, equivalent to a considerable money loss. With the low price of iron, this subsidiary product is worthy of attention. As I write, a Welsh smelter has visited me, to say that he has adopted this suggestion with advantageous results. I might

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adduce other improvements introduced by chemistry in the smelting process; but these will suffice to shew you that she has added to human power by increasing production, while she has also economized both the time and the materials employed.

2. *Soap.*—Soap is probably not older than the Christian era; for the soap of the Old Testament seems to have been merely alkali. Profane history, previous to Christ, does not allude to soap; and in all the detailed descriptions of the bath and of washing, it is never mentioned. Pliny describes its manufacture, but ascribes to it as singular a use as that given to the potato by Gerarde, who, in his "Herbal," assures us that it "is a plant from America, which is an excellent thing for making sweet sauces, and also to be eaten with sops and wines." So Pliny, in regard to soap, states, that its main purpose was to dye the hair yellow, and that men used it for this purpose much more than women. Gradually its use became more extensive, and its manufacture considerable. Soap generally consists of a fatty acid, combined with the alkali of soda. This soda was imported from Spain under the name of barilla, itself the ashes of plants grown near the sea. As these plants derived their soda from the sea, near which they flourished, chemistry, though singularly enough in the person of Napoleon Bonaparte, suggested that it might be artificially made from sea salt. A process for this was perfected, and soda derived from salt has now replaced barilla. From 1829 to 1834, the average annual import of barilla was 252,000 cwt.; it is now almost nothing. But besides this substitution, the cheapness and comparative purity of the soda made from salt is so great, that the manufacture of soap, and consequently of soda, is enormously increased, and probably exceeds ten times the largest quantity of barilla ever imported in one year into this country. Its cheapness and excellence have also had a prodigious effect on the manufacture of glass.

3. *Perfumery.*—Much aid has been given by chemistry to the art of perfumery. It is true that soap and perfumery are rather rivals, the increase of the former diminishing the use of the latter. Costly perfumes, formerly employed as a mask to want of cleanliness, are less required now that soap has become a type of civilization. Perfumers, if they do not occupy whole streets with their shops, as they did in ancient Capua, shew more science in attaining their perfumes than those of former times. The Jury in the Exhibition, or rather two distinguished chemists of that Jury, Dr Hoffman and Mr De la Rue, ascertained that some of the most delicate perfumes were made by chemical artifice, and not, as of old, by distilling them from flowers. The perfume of flowers often consists of oils and ethers, which the chemist can compound artificially in his laboratory. Commercial enterprise

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has availed itself of this fact, and sent to the Exhibition, in the form of essences, perfumes thus prepared. Singularly enough, they are generally derived from substances of intensely disgusting odour. A peculiarly fetid oil, termed "fusel oil," is formed in making brandy and whisky. This fusel oil, distilled with sulphuric acid and acetate of potash, gives the oil of pears. The oil of apples is made from the same fusel oil by distillation with sulphuric acid and bichromate of potash. The oil of pine apples is obtained from a product of the action of putrid cheese on sugar, or by making a soap with butter, and distilling it with alcohol and sulphuric acid, and is now largely employed in England in the preparation of the pine apple ale. Oil of grapes and oil of cognac, used to impart the flavour of French cognac to British brandy, are little else than fusel oil. The artificial oil of bitter almonds, now so largely employed in perfuming soap, and for flavouring confectionary, is prepared by the action of nitric acid on the fetid oils of gas tar. Many a fair forehead is damped with eau de millefleurs, without knowing that its essential ingredient is derived from the drainage of cow-houses. The winter green oil, imported from New Jersey, being produced from a plant indigenous there, is artificially made from willows and a body procured in the distillation of wood. All these are direct modern appliances of science to an industrial purpose, and imply an acquaintance with the highest investigations of organic chemistry. Let us recollect that the oil of lemons, turpentine, oil of juniper, oil of roses, oil of copaiba, oil of rosemary, and many other oils, are identical in composition; and it is not difficult to conceive that perfumery may derive still further aid from chemistry.

IV.—Professor Lindley.

1. *South Australian Wheat.*—If we take the subject of wheat, which perhaps will be regarded by many as paramount to all others, I think it will appear that there are some circumstances connected with this Exhibition which particularly deserve to be brought under public consideration, and especially one which, although the corn-factors in Mark Lane are familiar with it, is by no means a matter of universal notoriety—the high character and excellence of the wheat that comes to us from our South Australian colonies. There is now before us a sample of wheat from Adelaide, for which we are indebted to the kindness of Messrs Heath and Burrows, which is probably the most beautiful specimen of corn that has ever been brought to market in any country. It is a white wheat, in which every grain appears to be, like every other grain, plump, clear-skinned, dry, heavy, and weighing—what may seem incredible to those who are only accustomed to common wheat—seventy pounds a bushel. And it appears that Adelaide is capable

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of yielding vast quantities of corn of this description, which takes the lead in the markets of this country over all other white wheat.

It is very true that from Spain there has come a similar kind of wheat of great excellence also, as is seen by this beautiful sample from Castile, from the Mayor of Medina del Campo, the weight of which is unknown, and not easy to estimate, because it is not a clean sample. This is certainly of great excellence also; but, independently of its being the produce of a foreign country, it is almost inaccessible to us, and therefore a matter of curiosity more than of practical value; because, owing to the difficulty of transport, it cannot at present come into the markets of this kingdom. If it could, considering that it sells in Old Castile at 24s. a quarter, it is not easy to say what might be the effect upon the English market of the introduction of any large quantity of it. We find, moreover, that similar quantities of wheat, growing in the same rich country of Spain, are vendible at much lower rates.

I have already said, that among the wheats produced at the Exhibition, that from our South Australian colonies is the best—that it is much the best. And here let me make a remark on that subject. It has been supposed that all we have to do in this country, in order to obtain on our English farms wheat of the same quality as this magnificent Australian corn, is to procure the seed and sow it here. There cannot be a greater mistake. The wheat of Australia is no peculiar kind of wheat; it has no peculiar constitutional characteristics by which it may be in any way distinguished from wheat cultivated in this country; it is not essentially different from the fine wheat which Prince Albert sent to the Exhibition, or from others which we grow or sell. Its quality is owing to local conditions, that is to say, to the peculiar temperature, the brilliant light, the soil, and those other circumstances which characterise the climate of South Australia in which it is produced, and therefore there would be no advantage gained by introducing this wheat for the purpose of sowing it here. Its value consists in what it is in South Australia, not in what it would become in England. In reality, the experiment of growing such corn has been tried. I myself obtained it some years since for the purpose of experiment, and the result was a very inferior description of corn, by no means so good as the kinds generally cultivated with us. And Messrs Heath and Burrows, in a letter which I have received from them this morning, make the same remark. They say, "For seed purposes it has been found not at all to answer in England; the crop therefrom being ugly, coarse, and bearded." The truth is, as was just observed, the peculiarities of South Australian wheat are not constitutional, but are derived from climate and soil. It appears, therefore, that wheat may be affected by climate, independently of its constitutional

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peculiarities, but it does not follow that wheat is not subject to constitutional peculiarities like other plants. There are some kinds of wheat which, do what you may with them, will retain a certain quality, varying but slightly with the circumstances under which they are produced, as, for example, is proved by some samples here, especially of Revitt wheat, of a very fine description, exhibited in the building by Mr Payne, and which is greatly superior to the ordinary kinds of Revitt that appear at market. This clearly shews that Revitt wheat of a certain kind and quality is better than Revitt wheat of a different kind, both being produced in this country; so that, circumstances being equal, we have a different result, owing to some constitutional peculiarity of race. To other examples of the kind I cannot at present refer, because time will not permit me to dwell upon such points.

2. *Tobacco.*—It is not to be disputed that the finest tobacco in the world comes, as is generally supposed, from the Havannah; this was demonstrated by the admirably manufactured samples exhibited by the house of Cabañas and Carbazal. But there is only a limited area in Cuba in which that tobacco is produced; so that whilst the Havannah tobacco may be of excellent quality in general, yet it is only that which comes from a certain part which is much better than any other. Don Ramon de la Sagra, who resided many years in Cuba, and published an important work on that island, has stated that this is undoubtedly the fact,—that the best Havannah tobacco is the produce of a very small area. The consequence is, that this little area is the only place known where the finest kind of tobacco can be produced, and we cannot look even to Havannah for it with great confidence, inasmuch as it is chiefly used in the island, or as presents, and a limited amount going into general consumption. Yet we found that the tobacco from Trinidad did not appear to be in any way inferior to that from Havannah. Whether or not there exist generally in the island of Trinidad conditions of soil, and other conditions favourable for eliciting the admirable qualities which the best description of Havannah tobacco has, I cannot say; but, for my own part, I entertain no doubt whatever that, in that part of Trinidad from whence the tobacco came which was exhibited in the building, a kind of leaf quite equal to the best Havannah tobacco might be grown. Soil, no doubt, and a variety of circumstances of that kind, have much to do with the quality of tobacco; otherwise we cannot account for the varying qualities of the samples produced from various countries. This is strikingly shewn by a remarkable circumstance: some of the best tobacco sent to the Exhibition came from the southern Russian provinces. It was fully equal to the best American tobacco, grown in America under favourable circumstances; it was tobacco of the highest class. Yet nobody

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could have expected that such would have been the case with Russian-grown tobacco. The fact, however, proved how much climate and soil have to do with the quality of tobacco, and that the summer climate of some parts of southern Russia is admirably fitted for the cultivation of this plant.

On the other hand, manufacture exercises a great influence over the quality of tobacco. In Algiers, where the climate is apparently most favourable, the quality is such that nobody could be found to go through the punishment (I must so call it) of smoking an Algerine cigar. Those cigars were not smokable, because they were badly prepared; for Algiers is a country apparently favourable to the growth of the plant, if proper means were taken to prepare the leaves.

Then, again, we found that some English-made cigars, are not to be distinguished from Havannah cigars. I would ask any gentleman who has the misfortune to smoke, to examine those cigars made by Lambert and Butler, of Drury Lane, and to tell me whether they are English or foreign—by the look. They are not distinguishable by external appearance; and I may add, that the method which has been employed in preparing them renders them of very great excellence—of much greater excellence, in fact, than many of the cigars imported from Havannah, and paying a ten-shilling duty as manufactured tobacco. Now, this is a subject of greater importance than at first sight may appear; for if we can succeed in making cigars of such quality in England, we immediately create a large demand for labour. The preparation of cigars is by hand labour, which no machinery can ever supersede; and when we recollect that, in the German Commercial Union, in the year 1842, 605,000,000* of cigars were made, it is not necessary to inquire how much labour was required for that production. But none of the Continental cigars were good, except what came from Portugal. Those of the German Commercial Union were very inferior to the best English-made cigars that were produced; and there is no doubt whatever that it is quite practicable to make cigars in this country which shall be undistinguishable in appearance, and not very distinguishable in flavour, from any except those first-class Havannah cigars which scarcely ever come into consumption. It is a matter of considerable importance to establish that fact, because it may open the way to the employment of poor people, whose physical infirmities render them unfit for harder labour. I need not say that cigar-making is very light work.

With respect to the Portuguese cigars, I have only this remark to make, they were of a very unusual quality. They are, I presume, made in Portugal from foreign tobacco—perhaps Brazilian.

* 604,898,200, according to official returns.

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They appear as if they had been high-dried. The flavour is unlike that of the best cigars we have, and resembles that of high-dried snuff. They are very pleasant, smoke exceedingly well, are mild, and of excellent flavour; but not of the same flavour as those we are in the habit of getting in this country. Our cigar-makers will do well to turn their attention to this kind of manufactured tobacco.

3. *Typha Bread.*—There is another very curious substance, for specimens of which we are indebted to the kindness of Sir William Hooker, who has sent it from the important Museum belonging to the Gardens at Kew. These are cakes of typha bread—this from Scinde—that from New Zealand—where they are articles of food, prepared from the pollen of the common reed, mace, or bulrush of those countries. The one which is from Scinde, and which is called there *boor* or *booree*, is made from the pollen of the flowers of the *Typha elephantina*, or elephant grass of the country. The other, which is called *hunga hunga* by the people of New Zealand, is obtained from another species of bulrush, called *Typha utilis*. I believe these are the only cases known of the pollen of plants being used for food under any circumstances whatever; and it is not a little curious that countries so far apart as Scinde and New Zealand should have the same most unusual kind of diet. It is also interesting to know that the value attached to this as an article of food is not imaginary; for it appears from the researches of chemists that the pollen of plants contains an azotozed matter, which, mixed with the starch existing in pollen in great quantities, and with other matters, will give a real nutritive value to this curious substance. Whether there is on record, in the history of ancient times, anything concerning food made from the flowers of bulrushes, I do not know; but this is certain, that the bulrush from Scinde, which yields the cakes standing yonder, is probably the same as that from which the basket was made in which the infant Moses was placed; for to this day, in Scinde, bulrushes are woven into baskets, of the very same nature as we may suppose them to have been in the days of Moses.

4. *Preservation of Vegetables for long voyages.*—Preserved samples of white and red cabbages, turnips, Brussels sprouts, and various other things, prepared according to Mason's process, were exhibited. As to the method of preserving them, it appears to be free from all objection. First, it is very cheap; secondly, as we are led to believe by persons in France who are well informed on the subject, it perfectly answers the purpose. The mode of preparing these vegetables is shortly as follows: They are dried at a certain temperature (from 104° to 118°), which is neither so low as to cause them to dry slowly, nor so high as to cause them to dry too quickly; if the last happens, they acquire a burnt taste, which destroys their quality.

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They lose from 87 to 89 per cent. of their water, or seven-eighths of their original weight, after which they are forcibly pressed into cakes and are ready for use. I saw, a year ago, the original of a letter from the captain of the Astrolabe, a French vessel of war, speaking in the highest terms of the supply of these vegetables for the use of that vessel during her voyage. The French navy generally mentions them in the most favourable terms; and no reason appears for doubting such statements. The specimens before you are, I repeat, seen under unfavourable circumstances. They ought to have been kept in tin and protected from the air; instead of which, they have been lying about more than nine months in the Exhibition building, where they have been exposed to considerable dampness. Yet they are not injuriously affected, although they are absorbing moisture, as must necessarily happen in a damp place, and which, if it were to continue, would spoil them. Now, I think this is a matter of more consequence than it may appear to be, for the following reason: It is usual to supply the navy with preserved food of different kinds; and I am informed by a distinguished officer of the Antarctic expedition under Sir James Ross, that although all the preserved meats used on that occasion were excellent, and there was not the slightest ground for any complaint of their quality, yet the crew became tired of the meat, but were never tired of the vegetables. This should shew us that it is not sufficient to supply ship's crews with preserved meat, but they should be supplied with vegetables also, the means of doing which is now afforded.

5. *Preserved Meats.*—Preserved meats are out of favour just now. We hear of little except condemned canisters, which the Admiralty, unfortunately, have in store. It is the more proper, then, to state, that the evidence before the Jury went to shew, that it is possible to preserve meat in canisters, without undergoing any change, for a great length of time. We had hashed beef, which was excellent, dating back to 1836: we had boiled beef fifteen years old, preserved in canisters, and many other specimens, none of which were changed. It is clear, therefore, that the canister process of preserving is good, provided you keep a sharp eye on the contractors, and upon those who act under them.

What is more important than all other preserved provisions, is the article to which I must next request attention. A great deal of interest was excited when the contents of the Exhibition first became known—and it did not diminish afterwards—by a certain meat-biscuit, introduced among the American exhibitions from Texas, by Mr Gail Borden. We were told that its nutritive properties were of a high order: it was said that ten pounds weight of it would be sufficient for the subsistence of an active man for thirty days; that it had been used in the American navy, and had been found to sustain the strength of the men to whom it had been given

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in a remarkable degree. Statements were made to us, which have since been corroborated, that it would keep perfectly well, without change, under disadvantageous circumstances. Colonel Sumner, an officer in the United States Dragoons, who had seen it used during field operations, says he is sure he could live upon it for months, and retain his health and strength. The inventor, he says, names five ounces a-day as the quantity for the support of a man; but he (Colonel Sumner) could not use more than four ounces, made into soup, with nothing whatever added to it. The substance of these statements may be said to amount to this, that Borden's meat-biscuit is a material not liable to undergo change, is very light, very portable, and extremely nutritious. A specimen, placed in the hands of Dr Playfair for examination, was reported by him to contain 32 per cent. of flesh-forming principles; for it is a composition of meat, the essence of meat, and the finest kind of flour. Dr Playfair stated that the starch was unchanged; that consequently there could have been no putrescence in the meat employed in its preparation, and that the biscuit was "in all respects excellent." It was tasted—I tasted it—the Jury and others tasted it; and we all found nothing in it which the most fastidious person could complain of: it required salt, or some other condiment, as all these preparations do, to make them savoury. This meat-biscuit, as I said just now, was reported to be capable of keeping well; and this might well be true, because no foreign matter had been introduced into its composition; there was no salt to absorb moisture, and nothing else to interfere with the property of flour, or of essence of meat. These biscuits are prepared by boiling down the best fresh beef that can be procured in Texas, and mixing it in certain proportions with the finest flour that can be there obtained. It is stated that the essence of five pounds of good meat is estimated to be contained in one pound of biscuit. That it is a material of the highest value there can be no doubt—to what extent its value may go, nothing but time can decide; but I think I am justified in looking upon it as one of the most important substances which the Exhibition has brought to our knowledge. When we consider that by this method, in such places as Buenos Ayres, animals, which are there of little or no value, instead of being destroyed, as they often are, for their bones, may be boiled down and mixed with the flour, which all such countries produce, and so converted into a substance of such durability that it may be preserved with the greatest ease, and sent to distant countries,—it seems as if a new means of subsistence was actually offered to us. Take the Argentine Republic; take Australia, and consider what they do with their meat there in times of drought, when they cannot get rid of it whilst it is fresh—they may boil it down, and mix the essence with flour (and we know they have the finest in the world), and so prepare a substance that can be preserved for

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times when food is not so plentiful, or sent to countries where it is always more difficult to procure food. Is not this a very great gain?*

V.—Professor J. F. Royle, M.D., F.R.S.

The Indian Collection a basis for Schools of Design.—That I may not appear singular, says Professor Royle, especially to people in India, in my estimation of the value of these Indian products, I would beg, before concluding, to adduce some unconnected and independent testimonies. For this I may first refer to the articles in *The Times*, which were distinguished as much by their talent as by their discriminative criticism. "Turning to the class, manufactured articles, we find the long-established industries of the Indian Peninsula asserting their excellence in a manner at once characteristic and extraordinary. The same skill in goldsmiths' work, in metals, in ivory carving, in pottery, in mosaics, in shawls, in muslins, and carpets, was attained by those ingenious communities which now practise them, ages and ages ago. Yet, in these things, which the natives of India have done well from time immemorial, they still remain unsurpassed."—(*April* 25.) And again, "Yet, in another point of view, these remarkable and characteristic collections have a value that can hardly be overrated. By their suggestiveness, the vulgarities in art manufactures, not only of England, but of Christendom, may be corrected; and from the carpets, the shawls, the muslins, and the brocades of Asia, and from much of its metallic and earthenware products, can be clearly traced those invaluable rules of art, a proper definition and recognition of which form the great desiderata of our more civilised industrial systems."—(*Times, July* 4.)

I may fitly conclude these quotations with an extract from a letter of the Government Committee, on the selection of articles for the use of the Schools of Design, addressed by J. C. Melvill, Esq., Secretary to the Honourable East India Company:—"We have to request that you will acquaint the Court of Directors, that, having duly examined the collection exhibited by the Court, we have found it to contain, beyond any other department of the Exhibition, objects of the highest instructional value to students in design, and that we have selected the accompanying list of articles from their collection, which we express a hope may be secured for the benefit of the Schools." The Committee selected about two hundred and fifty. As some belonged to private individuals, they were able to purchase nearly two hundred articles out of the Indian collection, for the use and improvement of the Schools of Design in this country.

* The agency for the sale of meat-biscuit in this country, is 2 St Peter's Alley, Cornhill.

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And we may add that, in the course of his remarks on the foregoing lecture of Professor Royle, and on the striking examples of Indian art and manufacture, which, by the kindness of the Court of Directors of the Hon. East India Company, were exhibited in illustration of it, Mr Owen Jones, the chairman, observed, that, with all the artists of England with whom he was acquainted, as well as with foreign visitors, he had found but one opinion, viz., that the Indian and Tunisian articles were the most perfect in design of any that appeared in the Exhibition. The opportunity of studying them had been "a boon to the whole of Europe." Many have been purchased by Government for the use of the Schools of Design, and will no doubt be extensively circulated throughout the country. But it is to be hoped, said Mr Jones, that they will do more than merely teach us to copy the Indian style. If they only led to the origination of an Indian style, he would think their influence only hurtful. "The time has arrived," he added, "when it is generally felt that a change must take place, and we must get rid of the causes of obstruction to the art of design which exist in this country. Ever since the Reformation, when a separation took place between religion and art, England has not had anything like a style of her own. In every country which is under the influence of a particular religion, there a peculiar style of art is created. Such is the case with the Mohammedans, Greeks, and others. There now seems to be a general feeling and desire for art, and something must be done. I think the Government may be induced to assist in forming schools throughout the country on a different footing from that on which they are at present established. We see in the ornaments and articles from India the works of a people who are not allowed by their religion to draw the human form; and it is probable that to this cause we may attribute their great success in their ornamental works. Here in Europe we have been studying drawing from the human figure, but it has not led us forward in the art of ornamental design. Although the study of the human figure is useful in refining the taste and teaching accurate observation, it is a roundabout way of learning to draw for the designer for manufactures. It is to be hoped, as this Society is assisting in the formation of elementary schools, that it may be able to find a better means of producing the result in question."

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Anatomy of Doris.

A paper was read in the Royal Society on March 4, 1852, entitled, "On the Anatomy of Doris." By Albany Hancock, Esq., and Dennis Embleton, M.D., Lecturer on Anatomy and Physiology in the Newcastle-on-Tyne College of Medicine in connection with the University of Durham.

The authors have proposed to themselves to describe the anatomy of the three genera typical of the three groups of the Nudibranchiate Mollusca. An account of the structure of Eolis has already appeared in the Annals of Natural History.

A detailed description is given of the anatomy of Doris, the following species of which have been examined, and are referred to in the paper: *D. tuberculata*, Auct.; *D. tuberculata*, Verany; *D. Johnstoni*; *D. tomentosa*; *D. repanda*; *D. coccinea*; *D. verrucosa*; *D. pilosa*; *D. bilamellata*; *D. aspera*; and *D. depressa*; but *D. tuberculata* of English authors has been taken as the type of the genera, and the standard of comparison for the rest.

Digestive System.—The mouth, in all the species, is a powerful muscular organ, provided with a prehensile tongue beset with silicious spines, which, when the tongue is fully developed, are arranged in a median and two lateral series. Certain species possess, besides, a prehensile spinous collar on the buccal lip, occasionally associated with a rudimentary horny jaw. The mode of development of the lingual spines is shewn to be the same as that of the teeth of the vertebrata.

The *œsophagus* varies in length; in some, it is dilated at the top, forming a crop; in others, it is simply enlarged previously to entering the liver mass. The *stomach* is of two forms; one, as in *D. tuberculata*, is very large, receiving the œsophagus behind, and giving off the intestine in front, and lying in advance of the liver; the other is received within the mass of the liver, and is very small. The *liver* in all is bulky, mostly bilobed, and variously coloured, and pours its secretion by one or more very wide ducts into the

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cardiac end of the stomach—a small laminated pouch. A rudimentary *pancreas* is attached in some species to the cardiac, in others to the pyloric end of the stomach. The *intestine* is short, of nearly the same calibre throughout, rather sinuous in its course, and terminates in a nipple-formed anus in the centre of the branchial circle.

The *reproductive organs* are, male, female, and hermaphrodite. The *male organs* consist of penis and testes; the latter is connected with the former and with the oviduct. The *female organs* are, ovarium, oviduct, and mucous gland. The ovarium is spread over the surface of the liver in the form of a branched duct with terminal ampillæ. The oviduct terminates in the mucous gland. The *androgynous apparatus* is a tube or vagina opening from the exterior into the oviduct, having one or two diverticular spermathecæ communicating with it in its course. On the right margin of the body, near the front, is a common opening, to which converge the three parts of the reproductive organs. The spermatozoa are developed within large and fusiform spermatophera, and are observed in the spermathecæ, oviduct, and ovary.

Organs of Circulation and Respiration.—The circulatory organs are, a systemic heart, arteries, lacunæ, and veins. The existence of true capillaries in the liver mass seems probable. A second heart, a ventricle, having a portal character, is also described. The systemic heart lies immediately beneath the dorsal skin, in front of the respiratory crown, and comprises an auricle and ventricle inclosed within a pericardium. In the systemic circle, the blood is returned to the heart without having passed through the special respiratory organ. It is that blood only which is returned from the liver mass that circulates through the branchiæ.

The authors conclude from their observations, that in the molluscs there is a triple circulation: first, the systemic, in which the blood propelled along the arteries to the viscera and foot is returned, with the exception of that from the liver mass, to the heart through the skin; there it becomes partially aërated, the skin being provided with vibratile cilia, and otherwise adapted as an instrument of respiration: se-

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cond, the portal, in which venous blood from the system is driven by a special heart to the renal and hepatic organs, and probably to the ovarium, where it escapes doubly venous, with the rest of the blood which has been supplied to these organs from the aorta, and which is therefore only singly venous, to the branchiæ: third, the branchial circulation, in which flows only the more deteriorated blood, brought by the hepatic vein, but in which also that blood undergoes the highest degree of purification capable of being effected in the economy, namely, in the special organ of respiration. This triple circulation has not yet, as far as the authors are aware, been described as existing in the molluscan sub-kingdom. From the fact of the blood in *Doris* being returned to the heart in a state of partial aëration, it is clear, they say, that this animal is, in this respect, on a par with the higher crustaceans; and from the blood arriving at the heart in the same condition, according to the researches of Garner and Milne-Edwards, in *Ostrea* and *Pinna*, the Great *Triton* of the Mediterranean, Haliotis, Patella, and Helix, it can scarcely be doubted that this arrangement will be found throughout the Mollusca.

From a consideration of the facts cited in the paper, it may be deduced that the skin or mantle is, in the mollusca, the fundamental organ of respiration, and that a portion of that envelope becomes evolved into a specialty, as we trace upwards the development of the respiratory powers.

Upon the dorsal aspect of the liver mass is a branched cavity, that of the *renal organ*, lined with a spongy tissue, and opening externally at the small orifice near the anus.

Organs of Innervation.—These are in two divisions,—one corresponding to the cerebro-spinal division, the other to the sympathetic or ganglionic system of the *vertebrata*. The existence of the latter, it is stated, is now, for the first time, fully established. The centres of the first system are seven pairs and a half of ganglia. Of the seven pairs, five are supra-œsophageal, two infra-œsophageal; the single ganglion belongs to the right side, and has been named *visceral*. There are three nervous collars around the œsophagus, one of which connects the infra with the supra-œsophageal. The total

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number of pairs of nerves from the œsophageal centres is twenty-one, and there are also four single nerves.

The sympathetic system exists, and is more or less demonstrable in the skin, the buccal mass, and on all the internal organs. It consists of a vast number of minute distinct ganglia, varying in size and form, the largest quite visible to the naked eye, of a bright orange colour, like the ganglia around the œsophagus, and inter-connected by numerous delicate, white nervous filaments, arranged in more or less open plexuses. This beautiful system is connected with both sets of œsophageal ganglia.

The authors having found the sympathetic nervous system in several species of *Doris*, in *Eolis papillosa*, and in *Arion ater*, believe it to exist in all the more highly organised Mollusca.

The supra-œsophageal nervous centres in the Mollusca are in some instances so concentrated as to have led to the idea that they form only one mass; in others the ganglia are more or less distinct, and separated from each other. *Doris* has been taken as the representative of one class, *Aplysia* of the other; and, on a comparison of both the supra and infra-œsophageal ganglia of these with each other, there has been found a close correspondence between them, with the exception of the visceral ganglion. The single one in Doris is represented in Aplysia by a pair of ganglia, situated in the posterior part of the body, near the root of the branchiæ. The supra-œsophageal ganglia in the Lamellibranchiata appear homologous with those of *Doris*.

Having determined the existence of a true sympathetic or organic nervous system in *Doris*, the authors feel themselves more in a position to trace a parallelism between the œsophageal nervous centres of these Mollusca and the cerebro-spinal system of the Vertebrata; and accordingly they find there is a strict analogy between them, even to the individual pairs of ganglia of which they respectively consist; the general result being, that the whole of the ganglia grouped around the œsophagus in these Mollusca answers to the encephalon, and a small portion of the enrachidion of the Vertebrata.

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Organs of the Senses.—The *auditory capsules* are microscopic, composed of two concentric vesicles, the inner enclosing numerous oval, nucleated otolithes. The *eyes* are minute black dots beneath the skin, attached by a pedicle to a small ganglion. They are made up of a cup of pigment, receiving from behind the nerve, and lodging in front a lens, having in advance of it a cornea, the whole enclosed by a fine capsule. The authors believe they have shewn the dorsal tentacles to be the *olfactory organs*.

The *organs of touch* are the general surface of the skin, but more particularly the oral tentacles or veil. *Taste* is most probably located in the lips and channel of the mouth; the tongue being a prehensile organ, and ill adapted as the seat of such a function.

In conclusion, the authors comment on the high organisation of the Doridæ, and express their belief that the genus, as at present understood, will require to be broken up into several groups.

On three important Chemical Discoveries from the Exhibition of 1851—(A.) *Mercer's Contraction of Cotton by Alkalies;* —(B.) *Young's Paraffine and Mineral Oil from Coal;*— (C.) *Schrötter's Amorphous Phosphorus.* By Dr Lyon Playfair, C.B., F.R.S.*

[The following statements and arguments were supplied by Dr Lyon Playfair, as embodying considerations which he desired to impress on the attention of the Members of the Royal Institution.]

It is incumbent on those who, like myself, have been connected with the Great Exhibition, to inculcate its teachings, in order that it may influence the future, by being a starting-point for industry. Unless it imparts new life to productive industry, it has failed in the attainment of its object, and will, in history, degenerate into the record of a gigantic show, fitted only to pander to an idle curiosity. All of us have, no doubt, examined it with a higher object, and have derived lessons varying in character and amount according to the opportunities which we enjoyed in their acquisition. Those who have attended to its teachings with regard to the comparative progress of manufactures in different countries,

* Meeting of Royal Institution, Feb. 27, 1852.

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owe it as a public duty to announce their convictions on a subject of such large social importance.

My official connection with the Exhibition has enabled me to give more attention to it than most of those whom I have the honour to address, and convictions unfavourable to our position, as an industrial nation, have impressed themselves with such force upon my mind, that you will not be surprised that I seize every opportunity of directing public attention to them. I have already done so in a formal manner, on two previous occasions, and I rather depart from the custom of bringing before you subjects of original research at these evening meetings, in order that I may advocate the necessity of a more intimate union between science and practice in this country, at an Institution, whose proudest boast it is to have largely advanced the discovery of abstract truths, while it has always encouraged, at the same time, their applications to the increase of human resources and enjoyments.

In this lecture, however, I shall rather urge this point as a natural consequence of the subjects chosen for illustration of my argument than by any doctrinal exhortations, because these are not needed to strengthen your general convictions.

Our nation has acquired a proud position among the industrial states of the world, partly by the discoveries of her philosophers, partly by the practical powers and common sense of her population, but chiefly by the abundance and richness of her natural resources. Our fuel is abundant and cheap, and our iron and the lime necessary for its production are associated with it, so that all three may be extracted together under the most favourable circumstances. These local advantages gave to our country enormous power of production, and, under the favouring influences of an accidental combination, it supplied its produce to the rest of the world. Circumstances remaining the same, our industrial position was secured, and we have been thus lulled into a fatal apathy; for conditions were in fact varying with great rapidity, and the world at large was passing through a state of remarkable transition.

Setting aside the questions of capital and labour, which are not adapted for discussion in this place, the progress of manufactures is made up of two factors, possessing very different values. One of these represents the raw produce,—the other, the intellect or science employed to adapt it to human wants. As civilization advances, the value of the raw material as an element of manufactures diminishes, while that of the intellectual element is much enhanced. Improvements in locomotion by sea and land spread over the world the raw material formerly confined to one locality; and a time arrived when a competition of industry became a competition, not of local advantages, but of intellect.

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It was obvious that when improved locomotion gave to all countries raw material at slight differences of cost, that any superiority in the intellectual element would more than balance the difference. The Continental States, acting on a perception of this truth, saw that they could only compete with English industry by instructing their populations in the principles of science. Hence have arisen, in their capitals, in their towns, and even in their villages, institutions for affording a systematic training in science; and industry has been raised from the rank of an empirical art to that of a learned profession. The result is seen in the fact that we now meet most European nations as competitors in all the markets of the world. The result is palpably forced upon us by our actual displacement from markets in which we had a practical monopoly. The result was obvious in the Exhibition, where we saw many nations, formerly unknown as producers, frequently approaching, and often excelling us in manufactures our own by hereditary and traditional right.

The teaching of the Exhibition was to impress me with the strongest conviction that England, by relying too much on her local advantages, was rapidly losing her former proud position among manufacturing nations; and that unless she speedily adopted measures to cultivate the intellectual element of production, by instructing her population in the scientific principles of the arts which they profess, she must inevitably and with rapidity lose those sources of power, which, in spite of the smallness of her home territory, have given to her so exalted a rank among nations.

With these convictions you will not be surprised that I have chosen subjects connected with the Exhibition, although I have no merit or part whatever in their discovery. I have selected them for the following reasons.

We have a great reliance on the practical sagacity or common sense of our population,—certainly superior to that of any part of Europe: but we have not strengthened it by communicating scientific knowledge to those who are entrusted with the exercise of this practical power; and hence this common sense, unaided by the rules of science, has gradually assumed a sway over our manufactures. In other words, conjectural judgments have usurped the place of systematic knowledge. Practice and science have been followed out separately, as having no immediate connection. This separation, and even practical antagonism, has been fatal to our progress in industry; for manufacturers, as a body, have ceased to perceive that abstract science forms the roots of the tree of industry, and that to separate them is to sever the tree from its roots. In order to restore vigour to our declining industry, it is essential that confidence in the powers of science should be imparted to practice, and that the latter should be taught that it is, even as a question of social policy, highly important to encourage discoveries in ab-

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stract truths, however apparently remote from practice; because science only benefits industry by its overflowings, arising from the very fulness of its measure.

Every abstract truth, in its due time, adds to human resources and enjoyments, and it is this text that I wish to inculcate from examples derived from the Exhibition. One of the last generalizations of the great Berzelius, was that of *allotropism*, a name only eleven years old, and fully explained by him only six years since; and yet this generalization, apparently at the time only of abstract interest, entirely remote from practical application, produced as fruit the three most original, and, I think, the most important, practical discoveries of the Exhibition.

Having thus introduced the subject of his lecture, Dr Playfair proceeded to offer certain examples of *allotropism*. It had long been known that bodies crystallised in two or more incompatible forms. Thus, carbonate of lime as arragonite crystallises in prisms; whereas as calcareous spar it crystallises in rhombs. Sulphur also crystallises in two incompatible forms; so does the garnet. This is termed *dimorphism*. When two such forms exist they are found to be maintained in unequal stability; it appears, in fact, as if one form was normal and the other forced or strained. Thus a prism of arragonite is subject to change into rhombs of calc spar; and sulphur crystallised by heat in oblique rhombic prisms passes in a few days into a mass of rhombic octohedrons. Not only may the chemical and physical characteristics of such dimorphous bodies differ, but their colour and their specific gravity. Thus, the sulphuret of iron ($Fe\ S_2$), when crystallised in cubes, is persistent in the air; but when occurring in a rhombic form, readily passes into copperas or sulphate of iron.

Applying the preceding remarks to non-crystallised bodies, it was equally found that many were susceptible of allotropic modification. Thus cinnabar and vermilion were of precisely similar chemical composition with the black sulphuret of mercury. Again, the sesquisulphuret of antimony might be black or orange. Iodide of mercury is commonly red; when heated, however, it passes into a yellow powder, which by simple pressure and rubbing with a hard body becomes red again. Sugar is a remarkable instance of a solid capable of assuming two allotropic states; as sugar candy it is crystallised, as barley sugar it is amorphous; yet the composition of sugar in either case is the same. Nor are liquids exempt from the strange state of allotropism,—sometimes indeed manifesting a condition even beyond allotropism (*isomerism*), and not allowing us to reconvert them to their primitive state. Thus the chemical composition of oil of turpentine, of rosemary, of lemons, of copaiba, are identical, yet no one of these bodies has hitherto been turned into the other. The steroptane of otto of roses is identical in composition with coal gas, yet chemists are unable to change one into the other. The term isomerism has been com-

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monly employed in relation to bodies of like atomic composition, and has reference to equality of parts. The term allotropism is a better denomination, and has reference to the condition of unlike properties.

The preceding remarks by Dr Playfair were introductory to an exposition of the respective discoveries of Mr Mercer, Mr Young, and Dr Schrötter.

(A.)—Mercer's process consists in bringing cotton fabrics in contact with a solution of soda (cold), or a solution of dilute sulphuric acid, by subjecting it to either of which processes cotton acquires certain remarkable properties. In the first place, the texture becomes very much corrugated, and hence proportionably finer; it also assumes acid properties, rendering it more capable of taking up dyes. The process of induction which led Mr Mercer to his final discovery was curious. He started from the point of investigating the laws which determined the flow of water at various temperatures through minute tubes. From water he proceeded to aqueous saline solutions; from tubes he proceeded to their equivalent, namely, closely-folded woven tissue. Selecting for this purpose a thick reduplication of calico, fold on fold, and employing on aqueous solution of soda, Mr Mercer found that, by passing the solution through the calico, soda was removed. This removal he attributed to the act of filtration; but, subsequently finding that mere immersion of the calico in the same solution effected a like result, he concluded that the result was due to an actual combination of the cotton with the soda—a calico-*ate* of soda (if the lecturer might be permitted that form of expression) was generated.

The result of this agency of soda was, as formerly remarked, a physical corrugation, and an acquisition of certain chemical qualities. The former change was evident to the eye. Dr Playfair exhibited two stockings, one of which being nearly double the size of the other, although both came equal in size from the loom. The difference had been occasioned solely by chemical not mechanical agency. Dr Playfair, in developing the numerous practical applications of this physical effect, shewed that, besides the most obvious one of producing a material of increased fineness, the cotton thus prepared was far more capable of being dyed. Hot soda solution would not answer; and this fact was remarkable, and had its analogue in those salts which deposited themselves anhydrous on boiling. Instead of soda sulphuric acid might be employed; in which case it formed, in combination with the cotton fibre, an easily decomposable conjugate acid.

(B.)—Some years ago Liebig stated that one of the greatest discoveries of chemistry would consist in converting coal-gas into a solid form, thus enabling it to be burned like a candle. This had, in a manner, been accomplished by Mr Young. About three years since, Dr Playfair drew the attention of Mr Young to a spring of

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mineral oil, containing paraffine, and occurring in a coal-mine in Derbyshire. The liquid had been extensively applied by Mr Young as a lubricating agent; a use which Reichenbach had long ago suggested. After a period, however, this spring ceased to flow, when Mr Young applied himself to an investigation of the theoretical conditions under which it might be artificially formed. This gentleman saw that it would be difficult to convert gas into an allotropic form, whereas it was evident that gas must first come from a solid; hence he hoped to succeed in procuring the body before it assumed its gaseous state.

The illuminating portion of coal gas consists chiefly of olefiant gas, and the latter is isomeric with solid paraffine. But the allotropism does not end here; the peculiar slow distillation of coals yielding solid paraffine, also yielded another isomeric or allotropic compound, in the form of a lubricating oil, besides the additional products of a burning oil, and naphtha.

Dr Playfair now explained, by the aid of a diagram, the slow distillation process of Mr Young, employed in generating his allotropic form of olefiant gas, and directed the attention of his audience to some candles made of coal paraffine on the lecture table.

(C.)—Schrötter's process of manufacturing amorphous or allotropic phosphorus was the third in Dr Playfair's series. The properties of phosphorus in its ordinary condition are well known. It is spontaneously inflammable and highly poisonous; whereas the amorphous or allotropic phosphorus is neither spontaneously inflammable nor poisonous. Hence its great use in the manufacture of lucifer and congreve matches; an operation which not only imperilled the premises wherein it is conducted, but also the lives of those conducting it, causing the most frightful and fatal disease of the jaws and facial bones.

Common phosphorus, when heated to about 460° or 480°, changes into the allotropic condition, but a slight increment of heat changes it back again. Hence the manufacture of this substance, on a large scale, is attended with difficulties which Dr Playfair had no doubt would be eventually overcome by the energy of Mr Sturge, the patentee. The specific gravity of ordinary phosphorus is 1·77—of amorphous phosphorus, 1·964. Common phosphorus is soluble in bisulphuret of carbon, whereas the amorphous variety is not. Common phosphorus bursts into flame when brought into contact with iodine, whereas the amorphous or allotropic variety does not. Common phosphorus is luminous at very low temperatures, whereas the amorphous variety only commences to be luminous at a temperature of 500° Fahr. In forming lucifer matches by means of allotropic phosphorus, there is experienced the difficulty that it does not ignite by friction; hence it has to be mixed either with chlorate of potash, oxide of lead, or sulphuret of antimony, when friction takes effect and generates flame.

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Having thus discussed the experimental portion of his lecture, Dr Playfair concluded as follows :—

These three practical discoveries, for I think they are entitled to be considered as such, and not merely as inventions, have emanated from men all highly educated in chemical science. It is a proud subject of praise and of congratulation, that the two first discoverers, Mr Mercer and Mr Young, have, by the aid of science, raised themselves from the position of working artizans to that of employers in works involving considerable capital in their prosecution. Science has been to them a true power, the more so as in the arts which they profess, the manufacturers have usually been men of technical and not of scientific knowledge. The very fact of their success is a convincing evidence of what an immense development our industry might receive, if its sons were able to take advantage of the knowledge which science is constantly showering down upon the world.

There is a wide chasm between the laboratory of the philosopher and the workshop of the manufacturer—a chasm which must be bridged over by those who understand the nature of the foundations on either side. In general it is not the duty of the philosopher to do this ; it is more important for social progress, that he should continue to benefit the world by new accessions of truth, leaving to others to apply them to the promotion of the comforts and happiness of the human race. If technical men become disciples of science, then their acquaintance with the wants and requirements of manufacturers would enable them to derive from its teachings the knowledge requisite to apply it to the desired ends. Science should roll on, as it does now, a mighty river, from the abundant waters of which streams may be derived to fertilize the lands over which they pass ; for in the course of nature, these overflowings are restored in the form of refreshing showers. Their beneficial effects will, however, depend upon the skill of those who construct the channels destined to direct the waters for the uses of industry.

It is no new truth that science should always be ready to benefit industry by instructing those engaged in it, rather than by directly uniting with it. This truth is as old as the mythology, where we find no celestial so beneficent to industrial arts as Minerva, although she always preserved an independent existence, notwithstanding the passionate wooing of Vulcan, the god of industry. We have had it inculcated by the sages of all countries, strongly enforced by our own Bacon, and eloquently advocated in the theatre of this Institution by Davy.

Abstract science could not, if it would, cause itself and industry to progress satisfactorily by means of its discovering philosophers.

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There must be other means of conveying its God-born truths to industry, in order to freshen and invigorate its existence. The results of continental success indicate that the true way for those requiring aid is to come to the fountain of knowledge and take that which they need.

I need not detain you longer on this subject, except again to urge you to consider what must be the result of the system of instruction pursued abroad. In addition to many provincial schools, France has two central colleges of arts and manufactures, in one of which 300 of the best youth of France commence their education in science just where our colleges leave off, and after two years, they are poured into the provinces to impart to industry the principles of science which they have there attained. Prussia, Austria, Russia, and the Northern States are encouraging the same kind of education, and even yet more extensively. Need we be surprised, then, that they are progressing so rapidly in manufactures, in spite of their dear fuel and machinery. Recollect that we *have* reached that state when in future the *competition of industry must be a competition of intellect.*

Is England in a prepared state to meet this intellectual competition? Have we adapted the system of instruction in our schools to the wants and necessities of the age? Has science, or a knowledge of God's works and God's goodness and wisdom, yet become an important part of the instruction of our sons of industry, or do we not, by an antiquated notion, preserve the idea that the classical learning of the thirteenth century is all-sufficient for the requirements of the nineteenth century?

These questions are truly important if we desire to see England keep her ground in the industrial struggle of nations. I wish not to underrate any branch of human learning; but I do vehemently desire to see banished from our schools the bed of Procrustes, to the dimensions of which our children are clipped or extended until they are so changed in their natural aspirations for science, that it is very difficult, in after life, to communicate that amount which is necessary for its application to industry. I need not say, therefore, that until scientific instruction be added to the general system of education of our youth, that England cannot expect to be foremost in the industrial race of nations.

Already we see our capital largely employed to import foreign talent into our manufactures, and by this, in many cases, we retain our superiority. But it does not require much acumen to perceive the wretchedness of this policy as regards the nation, which, careless of the education of her own sons, sends her capital as a premium to the advancement of that intellectual knowledge in foreign states, who use it as the means of her destruction.

Excuse me if I have expressed my convictions on these points more strongly than you feel them; but they have taken such strong

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hold on my mind, that I connot see safety for the future of our nation unless by a great and comprehensive improvement in the instruction of her people. I shall conclude in the language of Dr Davy, when he addressed you on the benefits conferred by this Institution both on science and on industry:—

"There is no country which ought so much to glory in the progress of science as this happy island. Science has been a prime cause of creating for us the inexhaustible wealth of manufactures; and it is by science that it must be preserved and extended. We are interested as a commercial people; we are interested as a free people. The age of glory of a nation is also its age of security. The same dignified feeling which urges men to gain dominion over nations, will preserve them from the dominion of slavery. Natural, and moral, and religious knowledge are of one family; and happy is the country, and great its strength, where they dwell together in union."

The Spiral Structure of Muscle and the Muscular Structure of Cilia, as determined by Dr MARTIN BARRY.

We rejoice to learn that our distinguished friend, the celebrated physiologist, Dr Martin Barry, has so far recovered from his long illness as to be able to resume his microscopical investigations, and has lately published an account of his renewed researches on the *spiral* structure of muscle.

In the year 1842, Dr Barry, in a memoir published in the Philosophical Transactions of the Royal Society, recorded his discovery of the *spiral* structure of muscle. Two or three to whom he had shewn the tissue with his own microscope believed what he wrote, but by most persons it was doubted, by some flatly denied. Dr Barry's eye-sight became affected, and for years that instrument remained unused.* But our friend's sight was at length improved. He was in the same city (Berlin) with the celebrated physiologist, Purkinje, and shewed him that muscle had a spiral structure, and added the very interesting observation, that *cilia are no other than*

* Some observers had also declared, that when Dr Barry saw spermatozoa within the ovum of the Rabbit, he saw too much. But since Dr Barry's announcement spermatozoa have been found in the ovum by others. In papers laid before the Royal Society, the author of one of them found them within the ovum of the *Ascaris mystax*, and the author of the other, who denied the fact, confessed that he was obliged to cry *peccavi*, others finding them for him within the ovum of the frog.

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little muscles. He now wrote a fresh paper, containing an account of his renewed researches on muscle and of the muscular character of cilia. Purkinje translated that paper into German and communicated it to Müller's Archiv, where it occupies sixty-eight pages 8vo, with numerous engravings. Although we cannot, from the number of the engravings, give this very valuable contribution to science a place in our Journal, we embrace this opportunity of recommending it to the particular attention of naturalists. They will find in it a confirmation of Dr Barry's observations published nine years ago, "that the structure of muscle is a *spiral* structure," and discover (not published before) that cilia have a truly muscular character and structure; this being demonstrable, not only in cilia from the gill of the oyster and the common sea mussel, but in those of the Infusoria. (But of course if any cilia are muscular, all cilia are so.)

Letter from Mr Stevenson Macadam to Professor Jameson, on M. Chatin's Observations on the General Distribution of Iodine.

PHILOSOPHICAL INSTITUTION,
EDINBURGH, 22*d June* 1852.

DEAR SIR,—A short time ago M. Chatin read a memoir before the French Academy of Sciences, in which he stated that he had detected the presence of iodine in the atmosphere, in rain-water, in soils, &c. His observations led him to believe that the greater or less quantity of iodine present in any one district, to a certain extent determined the presence or absence of goitre and cretinism. With the view to corroborate, so far as possible, the results announced by M. Chatin, I have, this summer, undertaken a series of analyses in reference to the general distribution of the important element in question.

I am not aware that Chatin has published a detailed statement of the processes which he pursued, or the reagents which he employed in his experiments; but from the mention which he makes of the good offices fulfilled by *potash* in arresting "the complete decomposition of the iodine compounds present in water," and by *carbonate of potash* and *carbonate of soda*,

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in rendering the iodine present in soils much more easily extracted, I was led to think that the fixed alkalies had been much employed by him. Accordingly, in my first experiments, I used the alkalies in their caustic condition for the purpose of fixing any free iodine or compound of iodine which I might encounter.

I commenced with an examination of the atmosphere. By the arrangements I employed the air was made to traverse, 1st, a tube containing slips of paper which had been previously dipped in a solution of starch, and, 2d, a double-necked gas bottle, containing about three ounces of a dilute solution of caustic soda. A continuous stream of air was drawn through the arrangement for some hours. This experiment was conducted in the morning; in the afternoon a stream of air was for several hours drawn through the same arrangement, caustic potash being substituted for soda. The starch papers did not exhibit the slightest coloration even when moistened with distilled water. The solutions of potash and soda, however, on being treated with starch and nitric acid at once exhibited the rose colour characteristic of the presence of iodine in small quantity. So far my experiments seemed to lead to the desired conclusion, but when I carefully tested portions of the original alkaline solutions which had not been subjected to the current of air, I found the iodine present in them in quantity, to all appearance, as great as it was in those portions I had used in my experiments. Wishing to trace back the iodine to its source, I tested samples of the carbonate of potash, carbonate of soda, and lime-shell, which had been employed in the preparation of the caustic solutions, and in all three iodine was present in perceptible quantity. Desirous also of making certain that the lime and alkalies used in my investigations were as pure as other commercial substances of the same kind, I procured various specimens from different sources, and in every sample which I have as yet subjected to examination, I have detected the presence of iodine. So far then as concerns the determination of iodine in the atmosphere, my experiments were of no value; the alkalies through which air was drawn undoubtedly contained iodine originally, and therefore no certain conclusion could be drawn as to the

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probability of their being more highly iodized by contact with the atmosphere.

In a subsequent experiment the alkalies were dispensed with, the air being drawn through, 1st, a tube with slips of starched paper kept somewhat damp; 2d, a gas bottle immersed in a freezing mixture; and 3d, a gas bottle containing a solution of nitrate of silver. A continuous current of air was kept up for fully five hours, commencing about midday, and extending to the evening. At the conclusion of the experiments the papers were not altered in the slightest degree; the gas bottle (2) contained about a quarter of an ounce of liquid; and the nitrate of silver (3) had not been perceptibly changed. Subsequent analyses shewed that neither the condensed liquid (2), nor the nitrate of silver (3), contained a trace of iodine.

Experiments made with large quantities of the rain water which fell in Edinburgh during the last and present months, have led me to the same negative results.

I am well aware that consequent on the evaporation of water from the surface of the ocean, portions of the salts contained in it are carried up and disseminated through the atmosphere, ready to be rained down upon inland places, and that in this way, iodine, most probably as iodide of sodium, will reach the air. I was accordingly confident that I should succeed in verifying Chatin's observations in a district so near the sea as that around Edinburgh; either, however, iodine is less widely distributed than it has appeared to be, or its recognition is more difficult than Chatin's papers would lead us to expect. It is greatly to be wished that this able observer would publish his entire process.

In conclusion, I would merely add that the presence of iodine in pearl ashes leads me to believe that this substance will be found more generally distributed in the vegetable kingdom than it has hitherto been supposed to be, and this opinion is strengthened by the fact that I have found an appreciable quantity of iodine in the ashes of charcoal.

I trust that an early opportunity will enable me to write a more lengthened paper on this subject. I remain yours sincerely,

STEVENSON MACADAM.

To Prof. JAMESON.

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Upon Animal Individuality. By THOMAS H. HUXLEY, F.R.S., R.N.*

The lecturer first briefly described the structure of the Diphydæ and Physophoridæ—pointing out the general conformity of these animals with the common Hydra.

They differ, however, in this important respect; that the body in which the eggs are developed is in Hydra, a simple process; while in the Diphydæ and Physophoridæ the corresponding body presents every degree of complication from this form, to that of a free-swimming, independent "Medusa."

Still more striking phenomena were shewn to be exhibited by the Salpæ. In this genus each species has two forms. In the example chosen these forms were the S. democratica, and the S. mucronata; the former is solitary, and never produces ova, but develops a peculiar process the "gemmiferous tube;" upon which, and from which, the associated Salpæ mucronatæ are formed by budding.

Each of these carries a single ovum, from which the first form is again developed.

The Salpa mucronata, which is thus produced from the Salpa democratica, is just as highly organised as the latter. It has as complete a circulatory, nervous, and digestive apparatus, and moves about and feeds as actively; no one unacquainted with its history would dream of its being other than a distinct individual animal, and for such it has hitherto passed.

But the Salpa mucronata has exactly the same relation to the S. democratica that the free-medusiform egg-producing body of Physalia or Velella has to the Physalia or Velella; and this free-medusiform body is homologous with the fixed medusiform body of Diphyes; which again is homologous with the semi-medusiform, fixed body of a Tubularia, and with the egg-producing process of the Hydra.

Now as all these bodies are homologous with one another, one of two conclusions is possible; either, considering the

* Read at Meeting of Royal Institution, April 20, 1852.

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Salpa mucronata to be an individual, we are logically led to look upon the egg-producing process of Hydra as an individual also, which seems absurd.

Or starting with the assumption that the egg-producing process of Hydra is a mere organ, we arrive at the conclusion, that the Salpa mucronata is a mere organ also: which appears equally startling.

The whole question appears to turn upon the meaning of the word "individual."

This word the lecturer endeavoured to shew always means, merely, "a single thing of a given kind."

There are, however, several kinds of Individuality.

Firstly, There is what may be called *arbitrary* individuality, which depends wholly upon our way of regarding a thing, and is therefore merely temporary: such is the individuality of a landscape, or of a period of time; a century, for instance.

Secondly, There is an individuality which depends upon something else than our own will or caprice; this *something* is a fact or law of co-existence, which cannot be materially altered without destroying the individuality in question.

Thus a crystal is an individual thing in virtue of its form, hardness, transparency, and other co-existent qualities; pound it into powder, destroy the co-existence of these qualities, and it loses its individuality.

Thirdly, There is a kind of individuality which is constituted and defined by a fact or law of succession. Phenomena which occur in a definite cycle are considered as one, in consequence of the law which connects them.

As a simple instance, we may take the individuality of the beat of a pendulum. An individual beat is the sum of the successive places of the bob of the pendulum, as it passes from a state of rest to a state of rest again.

Such is the individuality of living, organised beings. Every organized being *has* been formless, and will again be formless; the individual animal or plant is the *sum* of the incessant changes which succeed one another between these two periods of rest.

The individual animal is one beat of the pendulum of life, birth and death are the two points of rest, and the vital force

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is like the velocity of the pendulum, a constantly varying quantity between these two zero points. The different forms which an animal may assume correspond with the successive places of the pendulum.

In man himself, the individual, zoologically speaking, is not a state of man at any particular moment, as infant, child, youth, or man; but the sum of all these, with the implied fact of their definite succession.

In this case, and in most of the higher animals, the forms or states of the individual are not naturally separated from one another: they pass into one another, undistinguishably.

Among other animals, however, nature draws lines of demarcation between the different forms; thus, among insects, the individual takes three forms, the caterpillar, the chrysalis, and the butterfly. These do not pass into one another insensibly, but are separated by apparent sudden changes; each change being accompanied by a separation of the individual two parts. One part is left behind and dies; it receives the name of a skin or cast: the other part continues the existence of the individual under a new form.

The whole process is called Ecdysis: it is a case of what might be termed *concentric* fission.

The peculiarity of this mode of fission is; that of the two portions into which the individual becomes divided at each moult, one is unable to maintain an independent existence, and therefore ceases to be of any importance; while the other continues to carry on all the functions of animal life, and to represent in itself the whole individuality of the animal. From this circumstance, there is no objection to any independent form being taken for, and spoken of as, the the whole individual, among the higher animals.

But, among the lower animals, the mode of representation of the individual is different, and any independent form ceases, in many cases, to represent the whole individual; these two modes, however, pass into one another insensibly.

The best illustration of this fact may be taken from the development of the Echinodenus, as it has been made known by the brilliant discoveries of Professor Müller.

The Echinus lividus stands in the same relation to its

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Pluteus as a butterfly to its caterpillar; in the course of development only a slight ecdysis takes place, the skin of the Pluteus becoming for the most part converted into the skin of the Echinus.

But in Asterias, the Bipinnaria, which corresponds with the Pluteus, gives up only a portion of its integument to the developed Asterias; the remaining and far larger portion lives for a time after its separation as an independent form.

The Bipinnaria and the Starfish, are as much forms of the same individual as are the Pluteus and Echinus or the caterpillar and butterfly; but here the development of one form is not necessarily followed by the destruction of the other, and the individual is, for a time at any rate, represented by two co-existing forms.

This temporary co-existence of two forms of the individual might become permanent if the Asterias, instead of carrying off the intestinal canal of the Bipinnaria, developed one of its own; and this is exactly what takes place in the Gyrodactylus, whose singular development has been described by Von Siebold.

But the case of the Gyrodactylus affords us an easy transition to that of the Trematoda, the Aphides, and the Salpæ, in which the mutual independence of the forms of the individual is carried to its greatest extent; so that even on anatomical grounds it is demonstrable that the difference between the so-called "skin" of the caterpillar, the free Bipinnaria, and the Salpa democratica is not in kind, but merely in degree.

Each represents a *form* of the individual; the amount of independent existence of which a form is capable, cannot affect its homology as such.

The Lecturer then proceeded to point out that the doctrine of the "Alternation of Generations" and all theories connected with it, rest upon the tacit or avowed assumption "that whatever animal form has an independent existence is an individual animal," a doctrine which he endeavoured to shew, must, if carried out, inevitably lead to absurdities and contradictions, as indeed Dr Carpenter has already pointed out.

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There is no such thing as a true case of the "Alternation of Generations" in the animal kingdom; there is only an alternation of true generation with the totally distinct process of Gemmation or Fission.

It is indeed maintained that the latter processes are equivalent to the former; that the result of Gemmation as much constitutes an individual, as the result of true Generation; but in that case the tentacles of a Hydra, the gemmiferous tube of a Salpa, nay, the legs of a Centipede or Lobster must be called individuals.

And if it be said that the bud must have in addition the power of existing independently, to constitute an individual; there is the case of the male Argonaut, which has been just shewn by H. Müller to have the power of detaching one of its arms (a result of gemmation) which then leads a separate existence as the Hectocotylus.

Without a misuse of words, however, no one would call this a separate individual.

In conclusion the lecturer stated his own views thus:

The individual animal is the sum of the phenomena presented by a single life: in other words, it is, all those animal forms which proceed from a single egg, taken together.

The individual is represented in very various modes in the Animal Kingdom: these modes pass insensibly one into the other, in nature; but for purposes of clear comprehension they may be thus distinguished and tabulated.

Representation of the Individual.

I. By Successive Inseparable Forms.
Ascaris. A. Forms little different = Growth.
Triton. B. Forms markedly different = Metamorphosis.

II. By Successive Separable Forms.

1. *Earlier Forms not Independent.*

Cockroach. A. Forms little different = Growth with Ecdysis.
Beetle. B. Forms markedly different = Growth with Metamorphosis.

2. *Earlier Forms partially Independent.*

Starfish.

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III. By Successive and Co-existent Separable Forms.

a. External Gemmation.	*b. Internal Gemmation.*
A. Forms little different.	All the forms produce eggs.
Nais. *Hydra.*	*Gyrodactylus.*
B. Forms markedly different.	Last forms only produce eggs.
*** Last Forms produced.	
Generally :	
Medusa.	*Fluke.*
Locally :	
Salpa.	*Aphis.*

These various modes of Representation of the Individual are ultimate facts. One is neither more nor less wonderful or explicable than another; any theory which pretends to account for the Successive and co-existent forms of the Aphis-individual must also account for the successive forms of the Beetle-individual or of the Horse-individual—since they are phenomena of essentially the same nature.

When the forms of the Individual are independent it becomes desirable to have some special name by which we may denote them, so as to avoid the incessant ambiguity of the two senses of the word individual. For these forms the Lecturer some time ago proposed the name "Zöoid." Thus the Salpa-individual is represented by two Zöoids; the Fluke by three; the Aphis by nine or eleven, &c.

The use of this term is of course a mere matter of convenience and has nothing to do with the question of Individuality itself.

SCIENTIFIC INTELLIGENCE.

1. *A Letter to Sir John W. Lubbock, Bart., F.R.S., "On the Stability of the Earth's Axis of Rotation." By Henry Hennessy, Esq., M.R.I.A., &c.* (*Communicated by Sir John Lubbock.*—The author refers to a communication to the Geological Society by Sir John Lubbock, in which he appeals, in support of the possibility of a change in the earth's axis, to the influence of two disturbing causes, which appear to have almost entirely escaped the notice of Laplace and Poisson, in their investigations on the stability of the earth's axis of

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rotation:—1. The necessary displacement of the earth's interior strata, arising from chemical and physical actions during the process of solidification. 2. The friction of the resisting medium in which the earth is supposed to move.

With reference to the first of these disturbing causes, the author states, that in his Researches in Terrestrial Physics (Philosophical Transactions, 1851, Part 2), he has been led to conclusions which may assist in clearing up the question. From an inquiry into the process of the earth's solidification, which appears to him most in accordance with mechanical and physical laws, he has deduced results respecting the earth's structure, which throws some light on the changes which may take place in the relation between its principal moments of inertia, which relation is capable of being expressed by means of a function which depends on the arrangement of the earth's interior strata.

He then states that he has found strong confirmation of his peculiar views respecting the theory of the earth's figure, in the experiments of Professor Bischof of Bonn, on the contraction of granite and other rocks in passing from the fluid to the solid crystalline state. From the results of these experiments, he has been led to assign a new form to the function, expressing the relation of the earth's principal moments of inertia. Referring to his paper for the mathematical processes by which he arrived at this result, he states that, from the theory he has ventured to adopt, it follows that, as solidification advances, the strata of equal pressure in the fluid spheroidal nucleus of the earth, acquire increased ellipticity, and each stratum of equal density, successively added to the inner surface of the solid crust, is more oblate than the solid strata previously found.

From these considerations alone, he remarks, it is evident that the difference between the greatest and least moment of inertia of the earth would progressively increase during the process of solidification. It follows, therefore, that if the earth's axis of rotation were at any time stable, it would continue so for ever. But, from the laws of fluid equilibrium, the axis must have been stable at the epoch of the first formation of the earth's crust; consequently, it continued undisturbed as the thickness of the crust increased during the several geological formations. Thus it appears that the displacement of the earth's interior strata, instead of having a tendency to change its axis of rotation, tends to increase the stability of that axis.

With reference to inequalities arising from the friction of a resisting medium at the earth's surface, the author observes that they could not exist, if, as in the manner here shewn, the axis of rotation coincided from the origin with the axis of figure.

In conclusion, he remarks, that if we could assume for the planets a similarity of physical constitution to that of the earth, the theorem as to the difference of the greatest and least moments of inertia of the earth would be applicable to all the planets; and thus we should

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be as well assured of the stability of our system, with respect to the motion of rotation of its several members, as we are already respecting their motion of translation.

In a postscript, referring to a third cause of disturbance in the place of the earth's axis of rotation, suggested in a letter from Sir John Lubbock, namely, the effects of local elevation and depressions at the earth's surface, the author states—If, with Humboldt, we regard the numbers expressing the mean heights of the several continents, as indicators of the plutonic forces by which they have been upheaved, we shall readily see that these forces are of an inferior order to those affecting the general forms and structure of the earth. If the second class of forces acted, so as not to influence in any way the stability of the earth's axis of rotation, the former class might, under certain conditions, produce a sensible change in the position of the axis. But when the tendency of the second class of forces is to increase the stability of the earth's axis, it would not be easy to shew the possibility of such conditions, as to render the operation of the other forces not only effective in counteracting that tendency, but also capable of producing a sensible change in the place of the axis of rotation.—(*Proceedings of the Royal Society, Feb.* 1852.)

2. *Influence of Oil on Water.*—Professor Horsford, at the Albany meeting of the American Association, read a paper, entitled, "On the occurrence of Placid Water, in the midst of large areas, where Waves were constantly breaking."

The Professor said he had noticed frequently that there were spaces of some extent, in places where the waves broke, which were very smooth; that though the swell, or rise and fall of the water, was just as great, yet there was no breaking of the waves, no white crest or comb, but he believed that these smooth spots were occasioned by oil, or oleaginous matter, which had accidentally happened to be spread on the surface at such places. To test this, he had, himself, when there was quite a stiff breeze, with waves on the surface of the water, which broke with considerable force off a comb or crest, emptied a phial of oil on the water from a boat. The effect was instantly seen. As far as the oil spread, the water was smooth, and the waves did not break; and what was very curious, the oil spread over the surface almost as rapidly to windward as it did to leeward. He had, therefore, inclined to the conclusion, that the smooth spaces, which might be observed in the midst of places where waves broke, were owing to the presence of oil, which might either come from decaying fish, or some other substance from which oil exuded.

Commodore Wilkes confirmed the statement and observations made by Professor Horsford. He cited the instance where he had seen the same effects in a violent storm off the Cape of Good Hope, from the leakage of a whale ship. He stated it was very curious

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to observe over what a great extent a small quantity of oil would produce the effect spoken of.

3. *The Salt Lake of Utah.*—Lieutenant Gunnison, of the Topographical Engineers, who has been employed for some time past in the survey of the great basin in which the Salt Lake is situated, speaks of the lake as an object of the greatest curiosity. The water is about one-third salt, yielding that amount on boiling. Its density is considerably greater than that of the Dead Sea. One can hardly get his whole body below the surface. In a sitting position, the head and shoulders will remain above water, such is the strength of the brine; and, on coming to the shore, the body is covered over with an incrustation of salt in fine crystals. The most surprising thing about it is the fact, that, during the summer season, the lake throws on shore abundance of salt; while, in the winter season, it throws up Glauber salt in large quantities. The reason of this is left to the scientific to judge, and also what becomes of the enormous amount of fresh water poured into it by three or four large rivers—Jordan, Bear, and Weber—as there is no visible outlet.

4. *Mud Volcano near the Salt Lake Utah.*—A correspondent of the Buffalo Commercial Advertiser gives the following description of a mud volcano in the vicinity of the great Salt Lake.

This volcano is in a plain of mud, and on the borders of the lake. It is composed of mud, and covers several acres. Steam and water are escaping from some half dozen apertures. The mud is raised up into cones, the highest not five feet from the general surface. They are terminated by tubes, some hardened and lined with crystals of sulphur and other substances. One of the cones throws steam and water ten or fifteen feet into the air. It escapes rapidly, and with a sound resembling the escape of steam from the pipe of a small steam-engine; and it ejects hot and cold water at short intervals. One caldron, some four feet across, boils up until it overflows; then sinks several feet, and again overflows. Nothing is seen but a mass of foam; the water is strongly impregnated with sal-ammoniac. There are other caldrons, from ten to twenty feet in diameter, filled to within three or four feet with boiling mud, which is occasionally thrown out in every direction. About a mile further off is another collection of mud cones; and on the opposite side, an island of volcanic rocks rises to the height of fifty feet; at the foot of it is salt in sheets, strongly impregnated with sal-ammoniac; that from the lake is pure, and is used by the Californians. In the vicinity of the volcano, we saw several ledges covered with pumice; and we met with it in various other places on the plains.—(*This, and the preceding, from the American Annual of Scientific Discovery for* 1852.)

5. *Mount Ararat.—On Mount Ararat; by M. Abich.*—The Great Ararat stands to the south-east of Little Ararat; and the two

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are situated in the longer axis of an elliptic volcanic system, and at the centres of the ellipse. This system occupies a plain, gently inclined to the north-east, which forms the natural slope between the high plain of Bajazed and that of the Araxes; the former 865 yards, the latter 1608 yards in height. The Great Ararat has in its upper part the form of a segment of a cone, slightly curved, and truncated towards the north-east, opening towards the Araxes. Ararat, viewed on the side of the Araxes, is a broad-backed mountain of imposing grandeur, owing to its breadth, and the wild features of the crater-shaped cavity it incloses; while on the other side it has the regular form of a pointed cone.

The gorge of St James impresses upon Ararat the appearance of a great crater of elevation. The interior structure of the mountain is here exposed to view, shewing trachytic rocks containing pyrites, arranged either in irregular beds or in extensive masses of conglomerates. There are, however, no modern lavas. These are found along the longitudinal axis of the system, and form two eruptive regions at opposite extremities of the line. That to the *north* includes a conical mountain of regular form, and the whole region (called Ripgoell) appears to be a product of successive volcanic eruptions. It terminates in a plain having a height of 3248 metres (3552 yards), which is richly covered with vegetation, excepting a lava region near its centre. Here are two vast crateriform cavities near the place of ejection, looking like deep gulfs of subsidence, and exposing a succession of layers of compact lava beds alternating with beds of scoriæ. On the *south* side, the volcanic action has opened a large gorge in the flanks of the great Ararat, which may be traced to the top, making an elongated niche or gorge, which extends downward and enlarges to its base. Below these is a plain like that of Ripgoell, with a similar crateriform pit or gulf. There is a long series of cones of eruption upon the eruptive band which communicates with the aforesaid gorge, several of which are regular cinder or scoriæ cones, and have ejected streams of lava. Other hills of scoriæ have no craters.

The part of the mountain between the two regions of eruptions is comparatively easy of ascent. M. Abich made his first attempt on the 16th August 1844, but was repulsed by a heavy storm. Again, on the 23d, he encountered another terrific storm, accompanied with electric discharges of great intensity; and so powerful was the electrical movement, that for a long time small phosporescent flames were seen going from the extremities of several metallic instruments, and fluttering from the iron heads of their canes whenever they gave them a vertical position. A fall of snow continued through the night till ten next morning, and covered the whole cone with sleet to more than a foot in depth, making the route extremely slippery, and that of their ascent thus far nearly impassable. A third attempt was

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made on the 3d of September; but was then unsuccessful, on account of the ice.

Again, on the 28th July 1845, M. Abich made his fourth trial, and reached the top at noon on the 29th. The top corresponds to the most elevated part of the west side of a great crater of elevation. This side has the character of a back, with a gently rounded and undulated surface, and varied with several low hills, running in a line nearly north-east and south-west. The two middle hills are the proper top of Ararat; the left one was visited by Parrot.

The neck between the great and little Ararat is low and flat, and includes a perfectly horizontal plain, about 550 yards broad. The debris on the summit of little Ararat consists of fragments of a rock, like the Andesite of South America.—(P. 265. March 3, 1851. *American Journal*, Vol. xiii., No. 38, Second Series, p. 269.)

6. *Mr Petermann and the Franklin Expedition.*—We have just received a copy of an interesting publication, entitled "The Search for Franklin; a Suggestion submitted to the British Public, by Augustus Petermann, F.R.G.S.; illustrated by a coloured chart of the Polar Basin." We regret, from the late hour when the publication reached us, we are prevented from entering into a detailed notice of Mr Petermann's proposal, for a spring expedition through the opening between Spitsbergen and Nova Zembla, as the best entrance into the Polar Basin, in some part of which, Mr Petermann thinks Franklin and his companions may still be found. We recommend this "Suggestion" to the attention of the geographical and general public.

7. *Phenomena of Vision.*—The special function with which the retina is endowed being the perception of light, a marvellous range of phenomena is open to the inquirer. It is indeed a wonderful thing to have ascertained beyond doubt, that in perceiving the tint of the scarlet geranium our eyes are affected by undulations recurring from four hundred and eighty-two millions of millions of times in a second: that before we can appreciate the tint of the yellow blossom of the gorse or laburnum, five hundred and forty-two millions of millions of vibrations must have taken place; and that to discriminate the colour of the violet, not less than seven hundred and seven millions of millions of movements must have been communicated to the fibrilæ of our retina! Whilst such facts almost transcend the powers of human conception, their establishment is a striking triumph of human intellect. But how great ought to be our admiration of that Omnipotence which has endowed the eye with the gift, not merely of appreciating one colour, but of distinguishing, in all their shades, the inexpressibly complicated vibrations which mark the hues of a parterre of flowers, and characterise the gorgeous plumage of the birds which give animation to a tropical forest. The sense of sight in its ordinary acceptation, may be defined as the recognition by the

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mind of certain impressions made upon the retina, and communicated through the medium of the optic nerve to the encephalon; a sound condition of each and all of these parts which may be considered as the media of communication, so far as one sense is concerned, between the external world and the mind, is indispensable for perfect vision. Light may fall upon the retina, and the images of objects may be there depicted; but should the optic nerve be unsound, or certain portions of the brain be disorganised, no responsive image is called up before the mind; the eye may gaze upon the noonday sun, but all is dark within.

The natural stimulus of the retina is the luminous rays; the appreciation of light and colour its active condition; and its state of repose suggests the appearance of darkness; but besides light, any other excitement applied to the retina or optic nerve gives rise to the same result, the production of luminous appearances. Pressure upon the eyeball, the electric current, or vascular congestion, all excite this special phenomenon. Occasionally, too, irritation of the brain has the same effect; and many are the waves and corruscations, the fiery clouds and flaming spectra, which haunt the amaurotic when certain morbid complications exist. The phantasms of fever, and the illusions of the dying, are to be placed in the same category with the above.—(*Todd's Cyclopedia.*)

8. *Vision under Water.*—Vision under water is attended with some curious consequences, the result of what is termed "internal" reflection. An eye placed under perfectly still water, as for instance, the eye of a diver, will see external objects only through a circular aperture (as it were) of 96° 55′ 20″ in diameter overhead. But all objects down to the horizon will be visible in this space; those near the horizon being much distorted and contracted in dimensions, especially in height. Beyond the limits of this circle will be seen the bottom of the water, and all subaqueous objects reflected and as vividly depicted as by direct vision; and, in addition, the circular space above mentioned, will appear surrounded with a rainbow of faint but delicate colours. In the eyes of fishes, the humours being nearly of the refractive density of the medium in which they live, the action of bringing the rays to a focus on the retina is almost entirely performed by the crystalline lens, which is nearly spherical, and of small radius in comparison with the whole diameter of the eye; there is also a very great increase of density towards the centre, whereby spherical aberration is obviated, the corneal refraction having little influence. —(*Ibid.*)

9. *Colours most frequently Hit during Battle.*—It would appear, from numerous observations, that soldiers are hit during battle according to the colour of their dress, in the following order:—Red is the most fatal colour; the least fatal, Austrian grey. The proportions are,—Red 12; Rifle green 7; Brown 6; Austrian bluish-grey 5.

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List of Patents granted for Scotland from 24th March to 22d June 1852.

1. To Colin Mather, of Salford, in the county of Lancaster, machine-maker, and Ernest Rolffs, of Cologne, in the kingdom of Prussia, gentleman, "certain improvements in printing, damping, stiffening, opening, and spreading woven fabrics."—March 24, 1852.

2. To Richard Archibald Brooman, of the firm of J. C. Robertson and Company, of 166 Fleet Street, in the city of London, patent-agents, "improvements in presses and in pressing in centrifugal machinery, and in apparatus connected therewith, part or parts of which are applicable to various useful purposes;" being a communication.—March 24, 1852.

3. To James Melville, of Roebank Works, Lochwinnoch, in the county of Renfrew, North Britain, calico-printer, "improvements in weaving and printing shawls and other fabrics."—March 29, 1852.

4. To Alexander Forfar, of Milnathort, in the county of Kinross, Scotland, builder, "improvements in ventilation, and in the prevention of smoky chimneys."—March 29, 1852.

5. To Joseph Jones, of Bilston, in the county of Stafford, furnace-builder, "certain improvements in furnaces used in the manufacture of iron."—March 29, 1852.

6. To Sir John Scott Lillie, Companion of the Most Honourable Military Order of the Bath, of Pall Mall, in the county of Middlesex, "certain improvements in the construction or covering of walls, floors, roads, footpaths, and other purposes."—April 2, 1852.

7. To William Watson Pattinson, of Felling, New House, Gateshead, manufacturing chemist, "improvements in the manufacture of chlorine."—April 2, 1852.

8. To George Mills, of Southampton, in the county of Hants, engineer, "improvements in steam-engine boilers, and in steam-propelling machinery."—April 2, 1852.

9. To Alexander Hediard, of Rue Tait-Bout, Paris, France, gentleman, "certain improvements in rotary steam-engines."—April 5, 1852.

10. To Joseph Pinlott Oates, of Lichfield, in the county of Stafford, surgeon, "certain improvements in machinery for manufacturing bricks, tiles, quarries' drain-pipes, and such other articles as are or may be made of clay, or other plastic substances."—April 6, 1852.

11. To Sturgis Russell, of No. 8 Bishopsgate Street, in the city of London, merchant, "improvements in weaving looms."—April 8, 1852.

12. To Richard Archibald Brooman, of the firm of J. C. Robertson and Company, of 166 Fleet Street, in the city of London, patent-agents, "certain improvements in the preparation and treatment of fibrous and membranous materials, both in the raw and manufactured state, in applying electro-chemical action to manufacturing purposes, and in the

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manufacture of saline and metallic compounds;" being a communication.—April 10, 1852.

13. To THOMAS BARNETT, of the town of Kingston-upon-Hull, in the county of the same town, grocer, "improvements in machinery for grinding wheat and other grain."—April 13, 1852.

14. To CHARLES WILLIAM SIEMENS, of Birmingham, in the county of Warwick, engineer, "an improved fluid meter, partly his own invention."—April 15, 1852.

15. To RICHARD ROBERTS, of Manchester, in the county of Lancaster, engineer, "improvements in machinery or apparatus for regulating and measuring the flow of fluids; also for pumping, forcing, agitating, and evaporating fluids, and for obtaining motive power from fluids."—April 16, 1852.

16. To WILLIAM WHITTAKER COLLINS, of Buckingham Street, Adelphi, in the county of Middlesex, civil engineer, "certain improvements in the manufacture of steel."—April 16, 1852.

17. To JOHN FLACK WINSLOW, of the city of Troy, in the State of New York, and United States of America, iron-master, "improvements in the machinery for blooming iron."—April 16, 1852.

18. To WILLIAM HYATT, of Old Street Road, in the county of Middlesex, engineer, "improvements in obtaining and applying motive power."—April 19, 1852.

19. To MARTYN JOHN ROBERTS, of Woodbank, Gerard's Cross, Bucks, Esquire, "improvements in galvanic batteries, and in obtaining chemical products therefrom."—April 19, 1852.

20. To FRANCOIS JOSEPH BELTRUNG, of Paris, in the Republic of France, engineer, "improvements in the manufacture of bottles and jars of glass, clay, gutta percha, or other plastic material, and caps and stoppers for the same, and in pressing and moulding the said materials."—April 19, 1852.

21. To JOHN WALTER DE LONGUEVILLE GIFFARD, of Serle Street, Lincoln's Inn, barrister-at-law, "improvements in fire-arms and projectiles."—April 19, 1852.

22. To WILLIAM GORMAN, of Glasgow, in the county of Lanark, North Britain, engraver, "improvements in obtaining motive power; which improvements, or parts thereof, are applicable for measuring and transmitting æriform bodies and fluids."—April 20, 1852.

23. To WILLIAM EDWARD NEWTON, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, "improvements in the method of, and apparatus for, indicating and regulating the heat and the height and supply of water in steam-boilers; which said improvements are applicable to other purposes, such as indicating and regulating the heat of buildings, furnaces, stoves, fire-places, kilns, and ovens, and indicating the height, and regulating the supply of water, in other boilers and vessels;" being a communication.—April 23, 1852.

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24. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "improvements in the manufacture of lenses;" being a communication.—April 26, 1852.

25. To Matthew Wilwin Sears, of 36 Burton Crescent, in the parish of St Pancras, in the county of Middlesex, commission agent, "the improved construction of guns and cannons, and manufacture of cartridges for the loading or charging thereof."—April 26, 1852.

26. To Stewart M'Glashan, of Edinburgh, Scotland, sculptor, "the application of certain mechanical powers to lifting, removing, and preserving trees, houses, and other bodies."—April 28, 1852.

27. To Thomas Bell, of Don Alkali Works, South Shields, "improvements in the manufacture of sulphuric acid."—April 28, 1852.

28. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "an invention for preventing the incrustation of steam-boilers; which invention is also applicable to the preservation of metals and wood."—April 28, 1852.

29. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "improvements in the method of manufacturing, and in machinery to be used in the manufacture of wood-screws, part of which improvements is applicable to the arranging and feeding of pins and other like articles; and also improvements in assorting screws, pins, and other articles of various sizes;" being a communication.—April 30, 1852.

30. To George Frederick Muntz Jun., of Birmingham, "improvements in the manufacture of metal tubes."—May 3, 1852.

31. To William Gillespie, of Torbane Hill, in the county of Linlithgow, Scotland, "an improved apparatus, instrument, or means for ascertaining or setting off the slope or level of drains, banks, inclines, or works of any description, whether natural or artificial, or under land or water."—May 5, 1852.

32. To William Thomas, of Exe Island, in the county of Devonshire, engineer, "certain improvements in the construction of apparatus and machinery for economising fuel in the generation of steam, and in machinery for propelling on land and water."—May 5, 1852.

33. To Julian Bernard, of Guilford Street, Russell Square, in the county of Middlesex, gentleman, "improvements in the manufacture of leather or dressed skins, of materials to be used in lieu thereof, of boots and shoes, and in materials, machinery, and apparatus connected with, or to be employed in such manufactures."—May 10, 1852.

34. To John Campbell, of Borofield, in the county of Renfrew, North Britain, bleacher, "improvements in the manufacture and treatment or finishing of textile fabrics and materials, and in the machinery or apparatus used therein."—May 10, 1852.

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35. To Richard Christopher Mansell, of Ashford, in the county of Kent, "improvements in the construction of railways, in railway rolling stock, and in the machinery for manufacturing the same."—May 10, 1852.

36. To George Leopold Ludwig Kufahl, of Christopher Street, Finsbury, London, engineer, "improvements in fire-arms."—May 11, 1852.

37. To David Dick, of Paisley, in the county of Renfrew, North Britain, machine-maker, "improvements in the manufacture and treatment or finishing of textile fabrics and materials."—May 11, 1852.

38. To Charles Ewing, of Bodorgan, in the county of Anglesea, steward and gardener, "an improved method or methods of construction, applicable to architectural and horticultural purposes."—May 11, 1852.

39. To Anthony Granara, of Leicester Square, in the county of Middlesex, hotel-keeper, "improved apparatus for lubricating machinery."—May 14, 1852.

40. To Clemence Augustus Murtz, of Manchester, in the county of Lancaster, manufacturing chemist, "an improvement in all preparations of every description of madder roots and ground madder, in or from whatever country the same are produced, also in munjeet in the root and stem from whatever country."—May 17, 1852.

41. To William Watt, of Glasgow, in the county of Lanark, North Britain, manufacturing chemist, "improvements in the treatment and preparation of flax or other fibrous substances, and the application of some of the products to certain purposes."—May 17, 1852.

42. To Peter Fairbairn, of Leeds, in the county of York, machinist, and Peter Swires Horseman, of Leeds, aforesaid, flax-spinner, "certain improvements in the process of preparing flax and hemp for the purpose of heckling flax, hemp, china, grass, and other vegetable fibrous substances."—May 17, 1852.

43. To William Edward Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, "improvements in the manufacture of coke, and in the application of the gaseous products arising therefrom to useful purposes."—May 19, 1852.

44. To John Harcourt Brown, of Aberdeen, in Scotland, and John Macintosh, of the same place, "improvements in the manufacture of paper and articles of paper."—May 24, 1852.

45. To Charles James Pownal, of Addison Road, in the county of Middlesex, gentleman, "improvements in the preparation and treatment of flax and other fibrous vegetable substances."—May 28, 1852.

46. To John Weems, of Johnstone, in the county of Renfrew, North Britain, tinsmith, "improvements in the manufacture or production of metallic pipes and sheets."—May 31, 1852.

47. To Alexander Johnstone Warden, of Dundee, in the county of

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Forfar, Scotland, manufacturer, "improvements in the manufacture of certain descriptions of carpets."—May 31, 1852.

48. To Joseph Swan, of Glasgow, in the county of Lanark, North Britain, engraver, "improvements in the production of figured surfaces, and in printing, and in the machinery or apparatus used therein."—June 10, 1852.

49. To George Searby, of Chelsea, in the county of Middlesex, decorator, being a communication from abroad, "certain improvements in apparatus for cutting and carving metal, stone, and other substances."—June 11, 1852.

50. To John Freerson, of Birmingham, "improvements in cutting shaping, and pressing metal, and other materials."—June 14, 1852.

51. To Thomas Twells, of Nottingham, manufacturer, "certain improvements in the manufacture of looped fabrics."—June 14, 1852.

52. To Andrew Fulton, of Glasgow, in the county of Lanark, North Britain, hatter, "improvements in hats and other coverings for the head."—June 14, 1852.

53. To William Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, being a communication from abroad, "improvements in machinery for weaving, colouring, and marking fabrics."—June 15, 1852.

54. To James Edward Coleman, of Porchester House, Bayswater, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in materials and apparatus to be employed in parts of railways, of engines, and of carriages, and in the application of such materials to those purposes, and to the manufacture of textile and other mechanism."—June 16, 1852.

55. To William Hindman, of Manchester, in the county of Lancaster, gentleman, and John Warhurst, of Newton Heath, near Manchester, cotton-dealer, "certain improvements in the method of generating or producing steam, and in the machinery or apparatus connected therewith."—June 16, 1852.

56. To Richard Archibald Brooman, of the firm of J. C. Robertson and Company, of 166 Fleet Street, in the city of London, patent-agents, being a communication from abroad, "a reaping machine."—June 17, 1852.

57. To William Gratrix, of Salford, in the county of Lancaster, dyer and printer, "certain improvements in the production of designs upon cotton and other fabrics."—June 17, 1852.

58. To James Edward M'Connell, of Wolverton, in the county of Bucks, civil engineer, "improvements in steam-engines, in boilers, and other vessels for containing fluids, in railways, and in materials and apparatus employed therein or connected therewith."—June 18, 1852.

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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Biography of Berzelius. By Professor H. ROSE of Berlin.*

On the 7th of August, in the memorable year 1848, died at Stockholm, BERZELIUS, after long and painful suffering, in his 69th year.

Distinguished men, who, during a long and active career, have enjoyed a great reputation, may have achieved this in various ways.

When a teacher, by his theoretical and practical instruction, by an overpowering force of convincing eloquence, draws round him a circle of students, whom, by his animating example, he inspires with enthusiasm for his doctrines,—or when, by an extraordinary talent for illustration, he renders even the most difficult branches of science accessible to the inquiring public, or when, by a talented combination of known facts, he opens the way to the most fruitful ideas, such a man may contribute to the general diffusion of a scientific spirit, and otherwise exercise the most beneficial influence. But when at the end of his career, we examine whether by his removal a void has been left, it will often be found that science would, upon the whole, have preserved the same boundary if he had not laboured for it. It will often be found that his influence upon science, although considerable, has only been indirect.

* Delivered at the Public Meeting of the Academie der Wissenschaften, in Berlin, on 3d July 1851; it will appear in the course of next year in the Memoirs of the Berlin Academy of Sciences.

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On the contrary, there are other specially-gifted men, who, possessing in a high degree the talent for investigation, recognise with marvellous penetration where the aid of experiment is needed, and frequently, by a few apparently very simple facts, clear up our views in a surprising manner, overthrow long-established prejudices, and advance science with gigantic strides.

It is rarely that men of the latter kind follow up and complete their scientific conquests. They generally content themselves with having shewn, by their discoveries, in what direction the study of details is to be pursued, and, after having pointed out the course and the method of filling up the gaps, resign the execution of the work to others.

Such a man was Humphry Davy. There are none who will not acknowledge that at the commencement of this century, chemical science received through him, by the discovery of the metallic nature of the alkalies, a most extraordinary impulse. But, however industriously and incessantly he occupied himself for years with experiments connected with his great discoveries,—experiments which led to the development of entirely new views, and enriched in a remarkable manner many branches of the science, still he was unable to cultivate it in its entire extent. During his unfortunately too brief career, while advancing from discovery to discovery, he had neglected to give his attention to the details of the science; and when he attempted to combine chemical facts into a system, and to publish an elementary work on chemistry, it soon became evident that he was not fully equal to this undertaking; and of his "Elements of Chemical Philosophy," only the first part of the first volume appeared.

But when a man possessed of the most remarkable powers of investigation enriches all branches of his science with the most important facts, distinguishes himself equally in empirical and in speculative researches, embracing the whole in a philosophic spirit, while, at the same time bringing the details into complete systematic order, and, lastly, occupying the lofty position of a practical and theoretical teacher of an inquiring circle of pupils, he fulfils the highest requirements

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of his science in such a degree as to remain in after ages a brilliant type of his class.

Such was Berzelius. It is seldom that all these qualities are found united in one man in so high a degree of perfection as they were in him. In this respect, and in chemical science at least, he has been exceeded by none.

Since the death of Berzelius several biographies of him have appeared, especially in Sweden. All tell how, in his childhood and youth, he had to struggle with care and poverty,—how he gradually overcame all obstacles,—how, in spite of his unpropitious external circumstances, he opened a way for himself, and entered upon the career for which he was destined.

In an academic eulogium, however, it is before all things appropriate to point out the scientific merits of the deceased member, to shew how much science has been extended by him, and how great is the loss it has suffered in his death.

It was exactly at the commencement of this century that Berzelius first appeared as an independent investigator. Volta had just constructed the electrical pile which bears his name, and its astonishing effects occupied in a high degree the attention of the men of science of the time. The unexpected chemical phenomena produced by the pile excited the interest of chemists fully as much as that of physicists, and induced them to multiply experiments with this remarkable apparatus. The first investigation made public by Berzelius was upon the effects of the electrical pile upon saline solutions. In the year 1803, there appeared in *Gehlen's Neuem. Allg. Journal der Chemie* an important paper on this subject, the joint production of Berzelius and Hisinger. However manifold and remarkable were the results which had hitherto been obtained with the Voltaic pile with regard to chemical decomposition, still no one had succeeded in discovering the laws of these phenomena. Berzelius was the first to find the thread which could lead with certainty through this labyrinth of complicated phenomena. He shewed that substances which are liberated at the one pole, have in other respects a certain analogy, that all combustible bodies, alka-

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lies and earths, were carried to the negative pole, and on the contrary, oxygen, acids, and highly oxidised bodies to the positive pole. But what particularly proves the admirable sagacity of Berzelius is the circumstance that he did not allow himself to be led astray by the appearance of the same body, sometimes at the positive, sometimes at the negative pole, as, for instance, the appearance in the decomposition of nitric acid, of nitrogen at the negative, in the decomposition of ammonia at the positive pole. It was, on the other hand, clear to him even at that time that the antitheses between the constituents of a chemical compound were only relative, and that one and the same body may behave as a base to a second, and as an acid to a third.

Three years after Berzelius had made this important investigation public, viz., in 1806, Davy developed similar views in a memoir upon some effects of electricity, which has become very famous. He extended the experiments considerably, prosecuted them with very ingenious apparatus, by means of which he succeeded in disproving many erroneous views as to the effects of the electric pile, which had at that time become general. He particularly explained the peculiar and remarkable mode of the transmission of substances from one vessel into another; but in his memoir he does not make any mention of the views of Berzelius, which corresponded with his own; and Pfaff, who translated Davy's paper for Gehlen's Journal, felt it necessary to remark that three years previously Berzelius and Hisinger had made known all the fundamental principles which Davy now brought forward as entirely new.

In 1807, Davy received the prize of 3000 francs, offered by the Emperor Napoleon for the best set of experiments made during that year on the subject of galvanism. The merits of Berzelius and Hisinger were unnoticed.

After Davy had made known, in October 1807, the important discovery of the metallic nature of the alkalies, and thus excited in a high degree the attention of scientific men, Berzelius also occupied himself with the separation of the alkaline metals by means of the voltaic pile. In the spring of 1808 he formed, at the same time as Seebeck, who was then

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living at Jena, the happy idea of employing mercury as the negative pole in the decomposition of the alkalies, which were placed at one side in contact with it in a moist state, and at the other in contact with the positive conducting wire. Berzelius made these experiments in conjunction with Pontin. In this way he succeeded in obtaining amalgams not only of potassium and sodium, but also of calcium and barium. Davy had in vain attempted to exhibit the metals of the alkaline earths by the methods which had fortunately yielded him the metals of the fixed alkalies; he could only obtain barium, strontium, and calcium, from amalgams prepared according to the method communicated to him by Berzelius.

But the most surprising results were those which Berzelius obtained on decomposing ammonia by the voltaic pile, likewise employing mercury as the negative pole. He obtained the ammoniacal amalgam, respecting the nature of which he even then entertained correct views. He had employed in this experiment caustic ammonia, while Seebeck, at the same time, and in a manner quite similar, obtained the amalgam with moistened carbonate of ammonia. Tromsdorf also, in conjunction with Göttling, obtained this amalgam at about the same time as Seebeck.

While at the commencement of his scientific career, Berzelius thus occupied himself in experimenting with the voltaic pile, he was also led to form a theory of this pile in some respects at variance with that of its famous discoverer. Volta, in constructing his theory, had not taken into account the chemical activity of the pile, regarding that merely as as an effect, and not as the cause of electrical action. Berzelius as a chemist put forward the opposite view, that the electricity of the pile results from the chemical action of the moist conductor and the positive metal. This chemical theory of the pile found great support, and it is still entertained by many distinguished physicists, and among them even by Faraday. Berzelius, however, unbiassed by prejudice, candidly returned again to the original view of Volta, after having convinced himself of its accuracy by a lengthened series of experiments. Long previously to the introduction of

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the batteries of Daniell and Grove, he had constructed one of zinc, copper, and two liquids, in such a manner that the zinc was not attacked by the liquid in contact with it, while the copper was briskly oxidised by the other. If now the oxidation of one of the metals were the cause of the electricity, the copper would have been positive, and the zinc negative; consequently the poles of the pile would be reversed. Before the circuit was closed, the copper was violently oxidised and dissolved; but, when the poles were connected, this action ceased immediately, and metallic copper was precipitated from the liquid upon the copper plate. This experiment rendered it obvious to Berzelius that chemical activity could not be the cause of the electrical phenomena; for the former ceased when the poles were connected, and the direction of the current was that indicated by the principle of contact-electricity. These experiments were instituted by Berzelius before many physicists had commenced to adopt the chemical theory of the pile, and especially long before Fechner endeavoured to prove the truth of the contact theory by his ingenious experiments.

It was not, however, these experiments with the voltaic pile which alone, or even principally, occupied the attention of Berzelius at the commencement of his career. At the instigation of Hisinger, who had a particular partiality for the chemical part of mineralogy, and to whom, as a geognost and mineralogist, Sweden owes so much, Berzelius early directed his attention to the quantitative analysis of minerals. He candidly admitted in after years, that in the first instance, when the law of combination in simple definite proportions was not yet established, he did this chiefly on Hisinger's account. But the very first result of an investigation of this kind, carried on in conjunction with Hisinger, was of the most brilliant kind: it was the discovery of a new metal, Cerium, during the year 1803, in the so-called tungsten of Bastnäs, near Riddarhyttan in Westmanland.

It must be admitted that the discovery of a new metal is often the result of mere chance. But it is not every chemist who is able, even when greatly favoured by chance, to re-

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cognise in a substance obtained in the course of an investigation, a hitherto unknown element. This requires such a perfect knowledge of the known elements as can only be acquired by many laborious investigations and long experience. For this reason new elementary bodies are not easily discovered by young chemists, not even by those of high talent. The discovery of cerium, which Berzelius made in his twenty-third year, shews therefore the great and rare sagacity which he displayed even in his first investigations.

Klaproth investigated the tungsten of Bastnäs simultaneously with Berzelius and Hisinger, and declared that the oxide which it contained in combination with silica was new. But he overlooked its metallic nature, and, although he obtained it of a reddish-yellow colour, regarded it as an earth, which he called Ochroit earth. The investigation of Berzelius and Hisinger was evidently carried out with more precaution than that of Klaproth. Not only did the latter overlook the partial solubility of the oxide in solutions of alkaline carbonates, he did not even remark the disengagement of chlorine on treating the ignited oxide with hydrochloric acid. It was not until afterwards, when, for the second time, he made known his investigations upon cerite in the fourth volume of his "Beiträge zur chemischen Kenniniss der Mineral Körper," which did not appear until 1807, that he mentioned the evolution of oxy-muriatic gas on treating the ignited oxide with muriatic acid, still, however, without attaching to the fact any great weight. Berzelius and Hisinger, on the contrary, justly considered this of very special importance, as unequivocally pointing out two different stages of oxidation, at that time one of the principal means of distinguishing between metallic oxides and earths, which were then regarded as simple bodies. Gehlen also directed attention to this point in a remark upon the paper of Berzelius and Hisinger. Further, he was fortunate enough to accomplish, with the aid of Hjelm, the reduction of the oxide, and to obtain the metal in an isolated, although not in a melted state.

When Berzelius subsequently undertook the determination of the equivalent weights of almost all the elementary bodies

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by means of a long series of experiments, he resigned the estimation of the equivalent of cerium to Hisinger, and did not occupy himself more specially with this metal. Thus probably the discovery of the oxides of two other metals, accomplished by Mosander, thirty-six years after that of cerium, escaped him.

Besides the examination of cerite, Berzelius undertook at that time the investigation of other new and interesting minerals. But during the first period of his scientific activity he was chiefly occupied in an entirely different branch of chemistry. Berzelius' first avocation in the world, by which, poor as he was, he had to earn his livelihood, was that of physician. He naturally sought in this profession for those occupations especially in which sound chemical knowledge was indispensable. Thus he examined several of the natural mineral waters of Sweden, and these investigations, although inferior to those of a similar nature which he subsequently carried out, especially that most excellent of them all on the Carlsbad water, still in every respect belong to the best of their time. In consequence of these investigations he established in Stockholm a manufactory for preparing these waters artificially.

But it was quite natural that as physician he should be induced to take up the study of animal chemistry. What he achieved in this branch of chemistry, and indeed within a short space of time, is extraordinary, opening as it were an entirely new field in this department of organic chemistry.

Before Berzelius' time animal chemistry was treated nearly in the same manner as that of inorganic bodies; the constituents of the animal body were arranged in certain classes, and described merely as objects of chemical decomposition, perhaps with a few general remarks as to their functions in animal life. This mode of treatment is, in a scientific point of view, totally valueless. Berzelius endeavoured to combine anatomical with chemical investigations, so as to tend to a common end, in order in this to give to experiments a higher scientific connection, and to direct the attention of the chemist to the physiological aspect of the subject.

In this spirit he investigated almost all parts of the animal body, solids and fluids, certainly only qualitatively, as at the

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commencement of this century there did not exist the most remote knowledge of those methods for the quantitative elementary analysis of organic substances, which have only been subsequently brought to a high state of perfection, especially by the labours of Berzelius himself, and afterwards by those of Liebig. But the investigations in animal chemistry carried out at that time by Berzelius, remain as examples for imitation at the present day, and have never been excelled. It is scarcely credible how much the very accurate results of his investigations differ from those which were obtained at the same time by other chemists, and solely because their investigations were undertaken from a one-sided point of view, and without any high scientific purpose. Beside Berzelius, the only chemist of that time who entered upon these investigations from a physiological point of view, was Fourcroy, but his results vary the most widely of all from those of Berzelius, since from scattered, uncertain, superficial, and often wholly incorrect observations, he drew general and extended inferences, although certainly in a very ingenious manner, and by his attractive illustration led the way to the greatest errors. In order to recognise the high superiority of Berzelius over Fourcroy in this respect, it is only necessary to compare the investigations of the latter upon blood, especially its red colouring matter, with that instituted by Berzelius on the same subject only a short time afterwards.

Berzelius made known his investigations in animal chemistry in the form of lectures, the first of which appeared in 1806; the second in 1808. Besides this, the most important examinations of separate animal substances appeared in the *Afhandlingar i Fysik, Kemi och Mineralogi*, and in Gehlen's Journal. He gave a masterly review of his labours in animal chemistry, compared with what was previously known on the subject, in a speech delivered upon the occasion of his vacating the presidentship of the Stockholm Academy of Sciences. It is there the custom annually to select from among the members of the Academy, a new president, who, in vacating his office, must deliver a scientific dissertation, which is printed. This is, indeed, frequently the only means of compelling members to publish their researches.

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Within the first period of Berzelius' scientific activity, two other investigations were carried out, which for that time were of the greatest importance. They were on the reduction of silica, and the composition of cast iron.

Although Berzelius had succeeded in obtaining the metals of the alkaline earths in combination with mercury, by means of the voltaic pile, he was unable to separate in a similar manner the radical of silica from its oxygen. In order, however, to satisfy himself that silica had a composition similar to the earths, he instituted a series of interesting experiments for the purpose of uniting the radical of silica with metals, especially iron, by mixing iron filings with carbon and silica, and exposing the whole to an intense heat, by means of which he obtained reguli which contained, together with silicium, carbon. He then found approximatively the quantity of oxygen present in silica, by estimating the quantity of iron and carbon, the latter certainly by a somewhat unsafe method. The remark which he makes at the close of his paper, published in 1810, is well worthy of notice. After having described his numerous experiments on the quantity of oxygen in silica, which throughout had not given very closely corresponding results, he concludes with these words: "I consider it moreover as unimportant to determine with precision the per-centage of oxygen or radical in silica, since I am unable at the present time to perceive either theoretical or practical advantage to be gained by this accuracy." A few years later he would not have expressed himself in this manner.

Another investigation important for this period was into the composition of crude iron. At the commencement of the present century, singular ideas of its composition had been formed. It was supposed that oxygen generally existed in iron associated with carbon; and, indeed, an Essay, in which the quantity of oxygen in crude iron was supposed to have been demonstrated, obtained a prize. This view was principally founded upon the circumstance, that on treating crude iron with non-oxidising acids, less hydrogen was obtained than with an equal weight of malleable iron. Berzelius proved that in this case an oleaginous hydro-carbon was produced,

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and shewed, with the greatest certainty, there could not possibly be any oxygen in crude iron. He determined the quantity of carbon by converting it into carbonic acid. Subsequently he attempted to estimate the carbon directly, by dissolving the iron through the agency of chloride of silver or chloride of copper. At that time attention had not been directed to the difference between the chemically combined and the mechanically intermixed carbon or graphite. This was not recognised until subsequently by Karsten, who also proved that graphite consisted of carbon alone, and contained no iron. While prosecuting the analysis of crude iron, Berzelius made several interesting observations; thus, among others, he was led to propose the use of benzoic acid as a means of separating peroxide of iron from protoxide of manganese and magnesia, instead of the then more costly succinic acid recommended by Gehlen. He further shewed, that on treating crude iron with nitric acid, an extractive substance, having the most perfect resemblance to the extract of vegetable mould, is produced from the carbon of the iron. He also accidentally discovered during this analysis the interesting double salt of persulphate of iron and sulphate of ammonia, the composition of which he was the first to determine qualitatively with accuracy. This salt he at first regarded as alum, on account of the form, though he found no alumina in it. He moreover pointed out that the silica which he obtained after the solution of the iron did not exist in the crude iron as such, but as silicium. However important the various facts ascertained during this investigation might be, still Berzelius was not fully satisfied with the results obtained, since he could not rely upon the correctness of the method which he had employed for the quantitative determination of carbon nor of magnesia, whose presence in the solution of crude iron he had proved. On this account, he published his investigation under the modest title of *Attempt to Analyse Crude Iron.*

I now come to the most important period of Berzelius' scientific activity. His previous achievements were owing more to fortunate chance than to any leading ideas. He was, to a certain extent, incited by the scientific interest of the time to take up the galvanic investigations, by the friendly

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intercourse with Hisinger to enter on the chemico-mineralogical, and, finally, by his professional position to engage in those on animal chemistry. Towards the end of the first decennium, however, he was led, especially by the investigations of Davy, to the study of the simple chemical proportions in which bodies generally combine with each other, and from this time applied all his energies to this subject. The activity which he now developed, under the guiding influence of a great idea, was in fact gigantic; for, after the lapse of only a few years, he established, to the amazement of his contemporaries, the complete theory of combining proportions, a subject the details of which he laboured constantly in perfecting and improving during the remainder of his life. It may safely be affirmed, that it was only from this time that chemistry became in truth one of the exact sciences; for from the collection of empirical facts which had hitherto borne the name of chemistry, the universal law now first developed itself, according to which bodies enter into chemical combination.

Berzelius is not, properly speaking, the first discoverer of the doctrine of chemical proportions. It generally happens in all sciences that great laws are not suddenly discovered by one investigator, but are gradually recognised.*

* * * * * * *

During the previous century chemists who had occupied themselves with the phenomena of the so-called chemical affinity, made several observations which incontrovertibly proved that there was a strict uniformity and order in the chemical combination of bodies. These men were especially Bergman in Sweden, Kirwan in Dublin, Wenzel in Dresden, and, above all, Richter in Berlin. The latter two had indeed come to the conclusion, that acids and alkaline bodies must combine in definite proportions, because in the double decomposition of neutral salts neutral products are formed.

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* Owing to the great indistinctness of the MS., a long sentence, which appears very involved and confused, is left out. The want of it, however, does not affect the sense of the text.

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But when it was attempted to prove this preconceived law by demonstrating the composition of the decomposed salts, all the proofs brought forward were either altogether insufficient or very incomplete,—a circumstance resulting from the then imperfect methods of separation, by means of which it was impossible to attain to such accurate analyses that the calculated results of the decomposition of two neutral salts could correspond with experiment.

The attention of chemists was soon diverted from this subject when in the eightieth year of the past century the theories of Lavoisier gave a new direction to the whole science. The attack upon the phlogistic theory, and the establishment of the antiphlogistic system, took undivided possession of all thinking minds. None had time to occupy themselves with any other than the qualitative changes which bodies underwent by their mutual decomposition. It was also necessary that Lavoisier's theory should have gained a complete ascendency before the doctrine of simple chemical proportions could be fully recognised and appreciated.

In addition to this, the development by Berthollet, one of the most gifted chemists of the time, of a theory apparently in total opposition to that of definite chemical proportions, tended to withdraw attention from the latter. Berthollet endeavoured to prove, that bodies which possess an affinity for each other are capable of combining in all proportions between certain maximum and minimum quantities, and that when the combination took place in definite proportions this was owing to special circumstances, particularly the power of crystallising or of cohering in any form, in consequence of which compounds could separate from a solution, as precipitates or crystals; or else owing to the expansion taking place on passing into the gaseous state, by which they removed themselves from the sphere of action of solid or fluid bodies. The most important law established by Berthollet was, however, that of the so-called chemical mass, according to which the deficiency of a body in chemical affinity may be replaced or made up for by increasing its quantity: and it is indisputable that this law, although it has latterly been more and more forgotten, is perfectly correct.

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The first of these principles, established by Berthollet, viz., that all chemical combinations are possible between a certain maximum and minimum, and in indefinite proportions, was immediately disputed by Proust, who endeavoured, by means of many ingenious experiments, to shew that every chemical combination took place in definite proportions, and that between it and the nearest allied combination there was a certain interval within which there was no intermediate stage.

Berthollet's views were at that time apparently supported by the numerous erroneous representations of the composition of the most important compounds. Likewise the experiments which he made, or caused to be instituted, in order to disprove the assertions of Proust were far from being adequate. Proust's experiments were certainly in most cases more correct, although not in such a degree as to place his views beyond all doubt.

However, sometime after the analogy in composition between the alkalies and metallic oxides was proved by Davy's important discovery, the attention of Berzelius was also drawn to the quantitative relations in which bodies combined with each other. It was the chemical nature of ammonia, which, in the first instance led him to enter upon this gigantic investigation. After the discovery of oxygen in the alkalies, the conjecture that all saline bases, and consequently ammonia, contained this element, was not unnatural. This view received a still greater probability by the discovery of the ammoniacal amalgam.

Berzelius now commenced a series of investigations for the purpose of determining the quantity of oxygen in alkalies and earths, by oxidising with water the basic metal in a weighed quantity of the amalgams which he first learnt how to prepare, then combining the oxide produced with hydrochloric acid, and in accordance with the then received views of the composition of chlorides, found the quantity of acid in the salt, and by the loss the quantity of oxygen in the base itself.

On subjecting ammonia to the same process he was not able either to isolate the ammoniacal metal, or to combine it

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with the mercury in such a quantity as to obtain a result. He then endeavoured to attain his purpose by the direct determination of the oxygen supposed to exist in ammonia. He wished to make an application of the discovery made by Bergman in his work "De diversa phlogisti quantitate in metallis," that when one metal separates another in a metallic state, from solution in an acid, the metal suffering solution yields precisely the same amount of phlogiston as the one previously dissolved requires in order to assume the metallic form; that a certain acid in dissolving metals expels equal quantities of phlogiston from the different metals; or to express the same in the language of the antiphlogistic system, that when a certain quantity of any acid combines with different metallic oxides, forming neutral salts, the oxides must contain an equal and invariable quantity of oxygen.

But in order to be able to apply this law of Bergman with perfect certainty, unassailable proofs of its perfect accuracy were necessary. Those, indeed, which Richter had given could not be regarded as at all admissible. Berzelius now compared his analyses of potash, soda, and lime with Bucholz's analysis of oxide of silver, and that made by my father of oxide of mercury; and he found in fact that the quantity of these bases which saturate the same quantity of hydrochloric acid, forming a neutral salt, contained, with very slight deviations, the same quantity of oxygen. But when he came to examine other metallic oxides and combinations with muriatic acids, the results obtained were so much at variance (perhaps on account of many erroneous premises which he assumed) with the principle of Bergman, that he was compelled to ascribe the want of correspondence either to his own want of dexterity in experimenting, or to an erroneous application of Bergman's laws. Since, however, careful repetition of his experiments gave results corresponding closely with those first obtained, he began to entertain doubts of the correctness of Bergman's law. When, again, he subsequently found that metallic sulphurets, on being perfectly oxidised by nitric acid, yielded neutral sulphates of the oxide, without any excess either of metallic oxide or sulphuric acid, he felt compelled to return again to the opinion which he had abandoned. At

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the same time he observed that metals unite with double (exactly or very nearly) as much sulphur as oxygen, and that by a simple rule of three, the capacity of combination of any metal for sulphur may be calculated from its oxide, or the contrary. He was now again led to study the mutual decomposition of neutral salts, and he finally succeeded in practically demonstrating, from the composition of several salts, the neutrality of those resulting from their decomposition.

I feel myself justified in mentioning the following circumstance, although somewhat of a personal character. Berzelius, after being unable to prove by calculation the neutrality of salts mutually decomposing each other, while basing his calculations upon the data of incorrect analysis, was often nearly abandoning the perplexing subject, but was induced by a paper of my father, upon the relation of the constituents of neutral muriates (published by him in 1806, a year before his death, in the 6th volume of "*Gehlen's Neuem. Allg. Journal*," p. 22), to persevere. My father had, in the first place, by at least one example, practically demonstrated, that by the decomposition of two neutral salts, muriate of baryta and sulphate of soda, according to his own analysis of them, and of the two salts resulting from the decomposition, and by calculation, results were obtained, which proved that the neutrality could not be disturbed.

Berzelius now considered it necessary, in order to attain to certain results, to investigate anew the composition of the most important compounds with extreme care, repeating the analyses several times before venturing to employ their results in the extension of his views. He remarked very justly, that, on account of the unchangeable neutrality of two salts decomposing each other, it was only necessary to analyse, with sufficient accuracy, all salts formed for example by sulphuric acid, and all those whose base is baryta, in order to be able, by a simple rule of three, to calculate the composition of all other salts, because these two series contain the three numbers which are necessary in order to find the fourth.

Berzelius now ventured upon an herculean task, which he prosecuted for many years with the most indefatigable in-

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dustry, and for a long time without any help. He re-examined every important chemical compound with the most admirable care and exactness. In this work, especially, he displayed rare talent, selecting, with the most extraordinary acuteness, those bodies which were the best adapted for investigation. He published an account of his labours, or rather the commencement of them, in the third part of the "*Afhandlingar i Fysik, Kemi och Mineralogi*" for 1810. They first appeared in German in 1811, in *Gilbert's Annalen*.

In these investigations, theory was constantly the touchstone employed to test the accuracy of the results, to attain which he was frequently obliged to vary his experiments almost endlessly. He was, in the first instance, compelled to improve the analytical methods, and to abandon many of those in use at that time, and by this means he was gradually led to those views which are now received by all chemists.

The most distinguishing characteristic of Berzelius's mode of working was, that with the most insignificant means at his command, he still succeeded in obtaining the most brilliant results. When he entered upon his great investigation, he was in possession of very small pecuniary means, he was in a condition almost of want, and without public support, which, considering the isolated situation of Sweden, must have been especially depressing and unfavourable. The difficulties against which he had then to contend were, in fact, enormous. At that time it was not possible to purchase in Stockholm pure reagents, as in Berlin; scarcely any chemical manufactories existed in the country, and, to import reagents from abroad, for instance from Germany, was often scarcely possible, on account of the difficulty of communication, especially during the war, and was at all times expensive and tedious. I have myself been a witness how Berzelius, even during the winter of 1820, while carrying out his important investigation of ferrocyanogen and ferrocyanide of potassium (which had long been procurable in Germany for a small sum per pound), was obliged to prepare this salt by the gramme, and indeed from a very bad material, the very impure Prussian blue of the shops. He was obliged to distil the spirit, the use of which in lamps he introduced, from ordinary brandy,

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and to prepare the most important acids himself, or to purify those which could be bought.

But it seems as if it was precisely those obstacles which would have discouraged and overcome any ordinary mind, that urged him on more perseveringly in his course. This is, moreover, a circumstance which has often occurred, and especially in Sweden. I need only call to mind Scheele, who almost made impossibilities possible.

Berzelius, in the first place, altered the methods of Klaproth, which at that time were the best, in so far especially that he employed considerably smaller quantities. The usual quantity operated upon by Klaproth and other chemists was rather more than five grammes; Berzelius never took more than two or three grammes, generally less, determining this quantity, of course, according to the nature of the constituents of the body to be examined. By employing more delicate balances, which Berzelius first introduced into use in chemistry, and by adequate care, results are obtained with a small quantity, which are at least quite as accurate, while they are obtained in much shorter time.

The spirit-lamp, with double draught, was likewise introduced into use by Berzelius. Formerly the ignition even of the smallest quantities of a substance was effected over a charcoal fire. He was also the first to make use of the small platinum crucible in which substances could be both ignited and weighed, and by the use of which considerably greater accuracy was insured, and the absorption of moisture as far as possible prevented. The filter containing the precipitate was always burnt when possible, and the ignited substance weighed together with the ash of the paper; a saving of time and trouble for which we are indebted to Mr d'Ohsson, who worked in Berzelius' laboratory. It was on this account that a paper was employed which left after combustion but a very minute quantity of ash, and which was made of excellent quality in Sweden, because there are springs there rising through granite, the water of which is almost free from fixed substances. The general introduction of this Swedish paper, to the manufacture of which Berzelius paid great attention, is also owing to him.

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The use of appropriate funnels, &c., as well as an immense number of other convenient applications originating with him, have contributed to render the results of analyses much more exact, and have much simplified the methods themselves.

Berzelius had moreover—and it is no slight merit—transferred chemical investigations in which charcoal fires were not necessary, from the damp kitchen, or cellar-like cold laboratory, into the comfortable dwelling-room. The present generation have scarcely an idea of the discomforts which were then connected with chemical researches. It certainly required no little scientific enthusiasm, during the severe winters of our northern climate, to remain in a place where there was the greatest absence of comfort, and which was even prejudicial to the health. But it was at that time thought that a laboratory with a stone floor was indispensably necessary even for trifling chemical operations.

The small caoutchouc tubes, by means of which experiments with gases may be so easily and safely conducted, and which, indeed, alone render many inquiries possible, were early employed by Berzelius in his investigations. Whoever has in former times conducted the disengagement of a gas will remember the unpleasantness of working with brittle glass tubes, and how easily an experiment miscarried from the slightest want of care. It was Berzelius who first rendered glass tubes, as it were, flexible, and they could then be employed in constructing the most complicated apparatus.

Possessing only the most scanty means, he was led to all those improvements by actual necessity. He took advantage of every opportunity to perfect himself in mechanical art. He was master of glass-blowing, which he learnt from a travelling Italian; he was familiar with turning, glass-grinding, &c. He made the greater part of his own instruments; and notwithstanding the isolated position of his native country, was thus enabled to construct those ingenious forms of apparatus by means of which he so infinitely advanced the study of chemistry.

I had the good fortune during my youth to assist the meritorious Klaproth in his chemical investigations, certainly only

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during his latter years, in the summer of 1816, when his labours were often interrupted by repeated attacks of illness. I was therefore enabled, while afterwards working for several years in the laboratory of Berzelius, to compare the different manner in which Klaproth and Berzelius worked. Their methods had exactly the same relation to each other as the respective accuracy of their results.

Even previously to Berzelius, Dalton had, in his new System of Chemical Philosophy, attempted to express numerically the relative quantities in which bodies generally combine, and, as he regarded bodies as consisting of atoms, by this means to establish as it were the relative weights of the ultimate atoms. Thus originated the doctrine of so-called atomic weights.

To Dalton, therefore, belongs the great merit of having given the correct idea of that which is now universally called atom in chemistry. Richter had previously employed in a similar sense the name relative mass (massentheil), in order to express the different quantities of acid and base which combine together forming salts; however, his idea was not so material as that of Dalton, and this character was necessary for giving it that perfect clearness, indispensable, if a theory was to be founded upon it. The long and obstinate opposition which was made to the idea of atoms, such as must be employed in chemistry, by German philosophers, and the war waged against the atomic view of the composition of bodies, with all the weapons of logical acumen, for a long time rather obstructed than favoured the advancement and spread of the exact sciences, especially chemistry. Now that the atomic theory is adopted by all, every one will certainly make use of the word atom, in order to explain the phenomena with ease and simplicity.

Dalton assumed that simple substances combined in equal atoms, and, indeed, atom with atom, when there was only one compound of the two elements; if several, one atom of one substance combined with one, two, three, or more atoms of another. The first conception of these so-called multiple proportions originated properly with Higgins, who made it known as early as 1789, in a work on the subject. But the

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most important experiments, by which the theory of Dalton was proved, were instituted by Wollaston, who published in the year 1814 his ingenious scale of chemical equivalents.

When the numbers made use of by Dalton are compared with those which Berzelius deduced from his own accurate experiments, differences are found similar to those existing between these latter and those given by Richter. The number of analyses upon which Dalton had founded his arguments was too small, and moreover they had not been executed with very great accuracy.

In the selection of a substance which should be taken as unity, so as to make the atomic weights found, comparable with each other, chemists hesitated between hydrogen and oxygen. Dalton chose hydrogen, and took it as $=1$ since its atom is the lightest of all the elements. Many chemists followed his example on this account, especially after Prout had subsequently attempted to shew that the atomic weights of all simple bodies were multiples of that of hydrogen. Richter had long before entertained a similar view, inasmuch as he assumed that the equivalents of all bases form an arithmetical, those of acids a geometrical progression. Nevertheless, Berzelius and Wollaston took oxygen as unity, because it was the most widely-distributed of all the elements, and existed in most compound substances. By adopting oxygen as unity, all calculations were greatly simplified. Berzelius took it as $= 100$, Wollaston $= 10$. Berzelius remained true to his opinion to the last, and always declared himself against that of Prout, even when in, 1840, it was again adopted by Dumas, who attempted to prove its truth for at least a few elements by actual experiment. It is true that the atomic weights of several of the non-metallic elements appear to be multiples of that of hydrogen, but it has not been possible to maintain Prout's views as regards others. So long as we are ignorant whether this correspondence is merely accidental, or actually a law of nature we must suspend a decision.

In the determination of the number of atoms in compound bodies, Berzelius proceeded with great caution. Dalton and others, who had put forward the view that substances combine, especially in such proportions that one atom of the one

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element unites with one atom of another, assumed also that when, for example, several oxides of an element existed, the oxygen atoms of the higher oxides were multiples of the oxygen in the lowest oxide. But when only one oxide was known, it was obviously very hazardous to assume that it consisted of equal atoms of both elements, without taking any notice of the other relations of this compound. Berzelius studied all the circumstances with the greatest attention; and the caution, as well as penetrating tact with which he proceeded, are evident from the fact, that, when subsequently, Mitscherlich, by his important discovery of Isomorphism, furnished an admirable means of recognising with certainty bodies having similar atomic composition, it was not necessary for Berzelius to make any alteration in his views.

Only upon one occasion did he feel himself compelled to modify his views regarding the arrangement of atoms in compound bodies. On the first establishment of his system, he was of opinion that in the simple compounds, such as oxides, there must be the most simple proportion, and that of two atoms of the radical to three of oxygen appeared to him to be too complex. Since in the oxides of iron the oxygen was in the proportion of two to three, he assumed that peroxide of iron consisted of one atom of metal and three atoms of oxygen, the protoxide and all those similar to it as consisting of one atom metal and two atoms oxygen. It was not until later in the year 1827, that Berzelius, particularly influenced by the proportions of the elements in the oxides of manganese, chromium, and sulphur, decided upon assuming, that, in the strongly basic, or so-called electro-positive oxides, there was but one atom of metal and one atom of oxygen, and, consequently, that the atomic weights formerly adopted by him must be reduced to one half. The higher oxides, such as peroxide of iron, would then contain three atoms of oxygen to two atoms of metal.

At that time, Berzelius adopted the view, that when a simple body is converted into the gaseous state, one volume of the gas corresponds to an atom. For this reason, water was alywas regarded by him as being composed of one atom oxygen and two atoms hydrogen. He held this opinion

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firmly, and disputed the hypotheses of Thomson, Dalton, and other chemists, who assumed that in two volumes of hydrogen there were as many atoms as in one volume of oxygen. Subsequently, when by the direct determination of the specific weights of sulphur, phosphorus, and mercury vapours, made by Dumas and Mitscherlich, this assumption of Berzelius was not generally confirmed, he limited its application to the permanent gases alone.

He was on this account compelled frequently to assume two atoms where other chemists assumed only one atom. He therefore introduced double atoms in those cases where they were the equivalent for one atom of another substance. Many chemists, especially in Germany, have not followed this view; and Leop. Gmelin, in the last edition of his "*Handbuch,*" as well as Liebig and his followers, have commenced to take the atomic weights of hydrogen, nitrogen, chlorine, bromine, iodine, fluorine, and phosphorus, as double those adopted by Berzelius, and many French chemists also entertain this view. The assumption that the so-called equivalents are identical in meaning with the term atoms, has indeed so much probability, that the agreement of so many chemists in this respect cannot be remarkable.

Notwithstanding this, Berzelius continued to the last to adhere to his old atomic weights, and the reasons which, in the last edition of his *Lehrbuch,* he has assigned for doing this, are so strong, that they cannot well be set aside. These he derives especially from the isomorphism of perchlorates and permanganates as proved by Mitscherlich, and from which it follows that a double atom of chlorine can replace a double atom of manganese. Since, however, manganese is in its compounds isomorphous with iron and chromium, for instance, in the alums, and since chromium in chromates has the same form as sulphates, a simple atom of chlorine must be able to replace an atom of sulphur. But if perchloric acid consists of a double atom of chlorine, combined with seven atoms of oxygen, then the hypochlorous acid contains only one atom oxygen, combined with the same quantity of chlorine as in the perchloric; and as hypochlorous acid consists of two volumes chlorine and one volume oxygen, the volumes of the two ele-

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ments must correspond with the simple atoms. Moreover, since it appears to have been proved, by often-repeated experiments with organic bodies, that their hydrogen can be replaced by an equal volume of chlorine, a simple, and not a double, atom of hydrogen must be able to replace one atom of oxygen or sulphur.

Even if it does sometimes happen that we not find conclusions of this kind confirmed by experience, if in the replacement of one body by another in compounds, an element, as for instance potassium, may be replaced by a compound radical such as ammonium, still it is not admissible to assume that such substitutions as may be theoretically inferred from the similarity in atomic weight, or atomic volume, really do take place, without the authority of repeated experiment. It is certainly convenient to regard equivalent and atom as synonymous terms, although not truly appropriate in a scientific view.

For the purpose of expressing the proportions in which bodies combine chemically, Berzelius, so early as the year 1815, employed certain signs as symbols for the different elements. Such signs were employed long before this in chemistry, or rather alchemy, although they were then of little value. These symbols undoubtedly owed their origin to the mysterious relation between planets and metals assumed by the alchemists, and the pleasure which they took in expressing themselves in a manner unintelligible to the people. Berzelius would not adopt the old symbols, not only because they were, in fact, destitute of all significance, but likewise because it is certainly easier to write the abbreviation of a word than to draw a figure. The symbols of Berzelius, however, serve to express the chemical combining proportions, and the chemical formulæ furnish a means of representing the numerical results of an analysis with all the simplicity of an algebraical formula.

The system of symbols introduced by Berzelius has met with such universal recognition, on account of its extraordinary convenience, that there is probably no chemist who does not now employ it; and this renders it the more remarkable, that the opposition made to this innovation was at first

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so considerable. A French philosopher exchanged the symbols proposed by Berzelius, for the initial letters of the French names for the elements. But it was in England that the greatest opposition was made to the adoption of the chemical formulæ of Berzelius. Even so late as 1822, an English chemist, speaking of them, said, "they are calculated more to produce misunderstanding and mystification than clearness, since they are of a nature totally different from algebraical formulæ; it would be easier to express oneself in ordinary words than with these symbols, which only make a kind of mathematical parade." Berzelius replied to the partly rude and uncourteous objections with dispassionate clearness and composure. Who would now consider it possible to dispense with the use of these "abominable symbols" of Berzelius, as they were termed by the editor of an English journal? The opposition to the introduction of these symbols was the more remarkable, since Dalton, in putting forward his atomic system in 1808, had felt the urgent necessity of representing the atoms of elements by means of symbols, which did not then meet with any opposition, although at the same time with no imitation in England. The symbols of Dalton are, however, far less appropriate than those of Berzelius; moreover they sufficed only to express simple combinations, and not very complicated ones. The introduction of Berzelius' symbols first enabled the chemist to construct chemical formulæ.

When Berzelius began to prove the law of chemical proportions by experiment, he was so firmly convinced that in inorganic bodies only the most simple relations obtained, that he even doubted the accuracy of his own experiments, when their results gave complicated relations. It was long before he could allow himself to admit that simple substances could combine with three, five, and seven atoms of oxygen, because these numbers were not multiples of each other. He therefore assumed, that in phosphoric acid there were four atoms of oxygen, in the arsenious and arsenic acid four and six atoms, and in oxide of antimony and antimonic acid the same number; and long after he had convinced himself of the elementary nature of chlorine, he doubted the

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correct statement of Stadion, that hyperchloric acid contained seven atoms of oxygen.

The examination of the oxides of nitrogen presented considerable difficulties to him. As ammonia was analogous to the fixed alkalies, and, under the influence of galvanic electricity yielded an amalgam with mercury, there was a possibility of assuming that this was a process of reduction, and that ammonia consisted of a metal and oxygen. But when ammonia was decomposed, no oxygen was obtained, but only nitrogen and hydrogen; the oxygen must therefore, Berzelius inferred, be concealed in these gases; and one or both must be oxides of the same radical, and this radical the metal ammonium. But if nitrogen alone were the oxidised body, then the metal ammonium must consist of the radical of nitrogen and hydrogen. Then, again, at that time several chemists, especially Gay-Lussac and Thénard, assumed that potassium and sodium contained hydrogen; however, in the controversy which arose on this point between these chemists and Davy, who sought to disprove their view, Berzelius immediately decided in favour of the latter, and supported him with very strong arguments. He also assumed, on this account, the presence of oxygen in hydrogen, and this as well as nitrogen were, according to his view, oxides of the metal ammonium. The different stages of oxidation were, according to him, the following: hydrogen, protoxide of ammonium (the present amidogen combined with potassium), ammonia, nitrogen, nitrous acid, nitric acid, and finally water, the highest oxide of the radical, which, however, must, on this view, have contained 72 times as much oxygen as the lowest oxide hydrogen.

Berzelius was led to adopt this extravagant but ingenious view by too great faith in the doctrine of proportions in the form in which he then conceived it. Somewhat later he retracted the opinion that hydrogen was an oxide, and demonstrated the elementary nature of this body by weighty arguments; but he still continued to regard nitrogen as containing oxygen, and endeavoured afterwards to prove this by means of its oxides. Even in 1818, in a paper upon the nature of nitrogen, hydrogen, and ammonia, he said, "I venture to assert, that the compound nature of nitrogen must

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not be regarded as a mere hypothesis, but, if the doctrine of definite proportions is admitted, as a demonstrated truth." He assumed that an unknown radical—nitricum—existed, to which he assigned the symbol N, subsequently retained for nitrogen, which was then regarded as the suboxide of this supposed radical, and the highest oxide—nitric acid—as containing six atoms of oxygen.

It was, however, in truth, the circumstance that the proportion of the oxygen in nitrous acid to that in nitric acid was as 3 to 5, which alone misled him so obstinately to assert the existence of oxygen in nitrogen, in which case that proportion would have been as 4 to 6. When a short time afterwards he made his researches on the composition of phosphorous and phosphoric acids, in which he found, almost simultaneously with Dulong, that the quantities of oxygen were in the proportion of 3 to 5, and after having in vain attempted to detect oxygen in phosphorus, his views respecting the compound nature of nitrogen were shaken, and he finally relinquished them, after having convinced himself that a similar relation obtained between very many, we may perhaps now say most, of the different oxides of simple bodies which form acids. Subsequently, he sometimes made the remark, without, however, assigning any particular importance to it, that from the production of nitrogenous compounds in the organisms of herbivorous animals, whose food frequently appears not to correspond in composition with them, the existence of oxygen in nitrogen might be inferred. However, in the last edition of his "Lehrbuch," even this remark does not occur.

This too great faith in the extreme simplicity of chemical combining proportions induced Berzelius, in some other instances, to assume the existence of oxides which had no reality. In the investigation of the oxides of tin, he assumed that the oxide obtained from the Spiritus Libavii, which certainly differs greatly in its characters from that obtained by means of nitric acid, was, in reference to the quantity of oxygen which it contained, intermediate between the protoxide and peroxide. Shortly afterwards Gay-Lussac shewed that it did not differ from the oxide prepared with nitric acid

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in its quantity of oxygen. After Berzelius had convinced himself of the truth of this remark, he shewed how much the two differed in their characters. This was the first example of Isomerism.

Berzelius connected the electro-chemical doctrine with that of simple definite proportions. It was very natural that he should apply the phenomena presented by the voltaic pile, and especially the facts which in his first paper he had so convincingly explained to the ordinary chemical processes. He assumed, that in every chemical process there was a neutralization of opposite electricities, in consequence of which heat and light were produced in the same way as in the discharge of a Leyden jar, the galvanic battery, or lightning, with the difference, that these phenomena were not always accompanied by chemical combination.

Even at the very first Berzelius did not conceal from himself the difficulties which this theory involved. He assumed that the atoms possessed electrical polarity, upon which depended the electro-chemical phenomena attending their combination. Thus the atoms of oxygen were regarded as having a preponderance of negative electricity; those of potassium a preponderance of positive. The unequal intensity of the electrical polarity in the atoms of different bodies, dependent partly upon their temperature, was regarded as the cause of the difference of force with which affinities are exercised. He altered his views of this subject at different times, and finally admitted that it was very possible that he was in error.

In classifying bodies as electro-positive and electro-negative, Berzelius regarded oxygen and the elements resembling it as electro-positive. Subsequently, however, he altered the nomenclature, and more correctly called them electro-negative. Oxygen alone he regarded as absolutely electro-negative, all other bodies being only relatively negative or positive, just as they would be related to each other when their compounds were exposed to the influence of the electric pile.

These views of Berzelius have been frequently disputed. And in truth, the phenomena attending the greater number

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of ordinary chemical processes, in which bodies act upon each other only when in immediate contact, are different from those which occur during the discharge of an electric pile where bodies act at a distance. It is only in some chemical processes, such as the arborescent deposition of metals, that there is a resemblance to the decompositions effected by the pile.

Much later Berzelius assumed the existence of another force, although only as regarded some special chemical changes—the catalytic force. The evolution of light and heat according to the electro-chemical theory could only result from the combination of bodies opposite in their characters; but when they occur on the decomposition of bodies, or when compounds are decomposed and new ones formed, without the body, whose presence causes this change, taking part in it, Berzelius ascribed this effect to the force of catalysis.

Much has been brought forward in opposition to the assumption of this new hypothetical force. But it is not justly censurable that, in an imperfect science like chemistry, all phenomena which stand isolated, for which no suitable analogues can be found, and which appear as it were wonderful, should provisionally be ascribed to a peculiar cause or force, so as openly to admit, that in the present state of the science it is more appropriate not to explain a chemical process at all than to do so in a forced and fastidious manner. With the advance of the science the number of phenomena belonging to such categories will always become smaller.

After Berzelius had laboured uninterruptedly during a space of ten years in the investigation of the atomic weights of the elements and their compounds, and had these so far established that all experiments corresponded to within small and unavoidable errors, he was in a position in 1818 to publish tables containing the atomic weights of about 2000 simple and compound bodies.

Thus had Berzelius completed as it were the scaffolding of his system, and he now commenced to supply the deficiencies which he had previously been obliged to pass over, and thus to plan out the whole.

Some time before, in 1814, he had also extended his inves-

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tigations to organic substances, and published a very important paper on the definite proportion in which the elements are combined in organic nature. He there shewed at length, that however different organic bodies might at first sight appear to be from inorganic, in regard to their elementary composition, still the only certain clue by which we could hope to arrive at a correct conception of the nature of the composition of those bodies which are produced under the influence of vital processes, was what was already known of the composition of inorganic bodies. He had therefore the great merit of having extended the doctrine of the simple chemical proportions in which bodies combine to organic bodies.

The first accurate experiments on the elementary composition of organic bodies had been instituted a few years previous to the appearance of this paper, by Thénard and Gay-Lussac, in 1811. Nevertheless, they contented themselves with drawing no other inference from their results than that a vegetable substance is always acid when it contains oxygen in a proportion greater than is necessary to form water; that, by an excess of hydrogen, resinous, oleaginous, or alcoholic substances were formed; and lastly, that when oxygen and hydrogen were present in the same proportions as in water, these substances were neither acid nor resinous, but analogous to sugar, gum, starch, milk sugar, or woody fibre. These conclusions were correct, only for the substances which they examined, and proved untenable when a greater number had been studied. From the results of their investigation of animal substances, they could not draw even similar inferences; they contented themselves with remarking, that they contained a greater quantity of hydrogen than was necessary to form water with the oxygen present, and that it was united with nitrogen in the form of ammonia.

Gay-Lussac and Thénard had burnt the organic substances by means of chlorate of potash, in a special form of apparatus; Berzelius borrowed from them the use of chlorate of potash, but his mode of combustion was incomparably more advantageous. He had already become convinced that it was necessary to estimate the carbonic acid obtained by its

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weight, and not by the volume. This was not always observed afterwards, on which account the analysis of organic bodies did not yield accurate results until a few years since, when Liebig introduced the extremely advantageous potash apparatus, which rendered it possible to weigh the carbonic acid with accuracy. Moreover, Berzelius estimated the hydrogen, not in the indirect way, like Gay-Lussac and Thénard, but he weighed it directly after it had been converted into water, which gave the results of his investigations a far greater accuracy in respect to this element.

The number of organic substances investigated by Berzelius was not very great, because the construction of apparatus, and the novelty of the subject, presented many difficulties. But although afterwards the methods of analysis were greatly simplified and improved, still the analytical results obtained by him in his investigation of organic substances have proved to be remarkably accurate.

He shewed that, not only the organic acid, but also the indifferent substances, combined with inorganic oxides in definite proportions, forming compounds resembling salts, by means of which their atomic weights could be determined, as in the case of inorganic bodies. This observation led to the view which regards organic bodies as oxides, whose radicals, however, are compound, while in the inorganic bodies they are simple. This view at first attracted little notice among chemists, and was not till long afterwards recognised as correct by many, after the number of fantastic ideas of the composition of organic bodies had created an earnest desire for a rational and consistent theory.

It cannot but be a subject of regret, that it was not granted to Berzelius to live to see several of the radicals hypothetically assumed by him, actually obtained, and indeed but a very short time after his death.

Soon after the establishment of the electro-chemical system, Berzelius applied the theory of chemical proportions to minerals, and put forward a mineral system, based upon chemical principles. If the minerals occurring in nature are regarded as having compositions similar to the substances artificially prepared in the laboratory, such a mineral system

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is, indeed, very appropriate. Every man of science must, however, admit, that in this case another system of classification must come into use in Mineralogy than is adopted in Botany and Zoology. The inorganic substances with which that science has to do consist of a large number—more than 60—simple bodies: the organic substances, on the contrary, of very few—only three or four. Since, moreover, the intimate connection existing between the chemical composition and all the external characters of minerals cannot be detected, it is obvious that minerals might be more easily and certainly recognised, distinguished, and classified, as soon as their chemical composition was studied; but not so plants and animals, in the case of which we do not yet know that there is such an intimate connection, and which, notwithstanding the greatest diversity in form, have almost all the same composition. Were it possible, likewise, to recognise their species by means of an easy chemical analysis, we should call every botanist and zoologist one-sided who neglected to avail himself of this means of recognition.

Before Berzelius' time it had often been attempted to classify minerals according to their constituents, but before the doctrine of definite proportions, and the correct views of the composition of bodies were known, this could only be imperfectly effected. Such systems were those which Karsten had put forward in his mineralogical tables, and Hauy, in his mineralogy, but the achievements of Berzelius in this respect, caused the attempts of his predecessors to be entirely forgotten.

The mineral system put forward by Berzelius met with opposition, especially from those who followed the so-called natural systems.

In the natural systems of mineralogy, the minerals are all placed according to their similarity in external characters. But all these systems differed from each other, because they were constructed in accordance with subjective principles.

Werner had, in addition, based his natural system, to a certain extent, upon chemical principles, which were not carried out very consistently, as indeed was impossible, considering the state in which the science then was. But Mohs put

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forward the fundamental principle, that mineralogists should only pay attention to the natural history characters of minerals, such as crystalline form, hardness, specific gravity, and not to such as cannot be observed without causing a sensible alteration in the substance. If it ever happens,—continues Mohs,—that a branch of natural history as mineralogy, employs such characters in its method as these last mentioned, it then exceeds its legitimate bounds, becomes entangled with other sciences, and hampered with all those difficulties of which mineralogy has long been a warning example.

(*To be concluded in our next.*)

Some Observations on the Ova of the Salmonidæ. By JOHN DAVY, M.D., F.R.S., &c. Communicated by the Author.

M. Vogt, in his able and elaborate work on the Embryology of the Salmonidæ, has pointed out a remarkable property belonging to the ova of these fishes, viz., that of having their fluid contents coagulated by admixture with water. —Thus, as he states, "Lorsqu'on crève un oeuf dans l'eau, on voit à l'instant même la masse entière du vitellus se transformer en une matière blanchâtre, lactée, opaque et filamenteuse, qui n'a plus aucune ressemblance avec la substance vitellaire de l'oeuf intacte. Voulant m'assurer si c'etait réellement l'effet de l'eau, j'ouvris un oeuf au foyer du microscope et j'y mêlai une goutte d'eau, pendant que j'observais le vitellus : partout où les deux liquides entrèrent en contact il en résulta à l'instant même une quantité de petite granules opaques, qui furent affectés pendant long temps d'un mouvement molleculaire très pronouncé. Ces granules etaient si petits que sous mon plus fort grossissement, ils ne m'apparurent que comme de petits points foncés et leur nombre considérable me prouva suffisament que ce n'etaient pas des nucléolules devenus libres par l'effet de l'eau qui auraient fait crever les parois des cellules."*

* Embryologie des Salmones, par C. Vogt, p. 11, in vol. i. of M. Agassiz's Hist. of Fresh-Water Fishes. Neuchâtel, 1842.

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The observations of M. Vogt were made principally on the ova of the Palée (*Coregonus Palœa*, Cuv.) of the Lake of Neuchâtel. Those which I have to offer, have been made in most part on the mature ova of the Charr of Windermere. These, it may be right to mention, are commonly spherical, about two-tenths of an inch in diameter, weighing about a grain each (the fluid contents about ·98 of a grain; the membranous shell about ·02 of a grain), of the specific gravity 1095, or thereabouts,—being suspended in a solution of common salt of this density. The contained fluid—the vitellus—is slightly viscid; of a light yellow hue, from oil particles of this colour diffused through it; and slightly alkaline, as indicated by its effects on test papers.

Having premised thus much, I shall briefly relate the results of the experiments which I have made; and, *first*, On the action of water on the vitelline fluid.

When about equal parts of the fluid of the egg and water were mixed, the result was an immediate coagulation, exactly similar to that described by M. Vogt in the instance of the vitellus of the Palée. If the proportion of water was very much less, the two fluids mixed without coagulation, either at the instant or afterwards. The mixture was capable even of dissolving a minute quantity of coagulum obtained by the action of a larger quantity of water. When a puncture was made in the egg under water, the little fluid that issued was instantly covered with a delicate pellicle, and was shortly wholly coagulated, as were also, gradually and pretty rapidly, the entire contents.

Secondly, Of the action of heat.—Contrary to what might have been expected, heat, even a temperature of 212° Fahrenheit, did not coagulate the vitellus. Eggs placed in a dry tube immersed in boiling water, shrunk and became shrivelled from evaporation, but not opaque; and when evaporation was arrested by the presence of steam, generated from accompanying moist cotton, even this change was prevented; after immersion of the tube from five to ten minutes in boiling water, the vitellus remained fluid, coagulable, however, as before, on admixture with water. Heated in water, the effect was strikingly different. At 160°

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Fahr., the coagulation took place pretty rapidly; at 120°, more slowly; and slower still at lower temperatures; at 100°, the time required for coagulation to take place was about half an hour. The higher the temperature at which the coagulation was effected, the greater was the firmness of the coagulum; at the boiling temperature, continued for a few minutes, it was as firm nearly as the yoke of the egg of the common fowl similarly treated. That, in all these instances, water penetrated and mixed with the vitellus can hardly be doubted; at 100°, it may be mentioned in confirmation, that the coagulation extended gradually, spreading almost from a point. These trials were made with unimpregnated eggs. Repeated on others that had been subjected to the influence of the spermatic fluid by admixture about thirty-six hours previously, the effect of coagulation was decidedly slower in taking place, *i. e.*, the fluid resisted longer incipient coagulation; but when it commenced, it seemed to proceed as rapidly in one instance as in the other.

Thirdly, Of the action of alkalies and salts.—Ammonia or potassa, or the sesquicarbonate of either alkali, in solution, added in very minute quantity to the fluid vitellus, did not prevent its coagulation; but, if of moderate strength, no obvious effect was produced, either at the instant of admixture or afterwards; moreover, if coagulated vitellus, obtained by the action of water, was added, a certain portion of it was dissolved.

Common salt, muriate of lime, muriate of ammonia, muriate of barytes, nitre, phosphate of soda, sulphate of magnesia, alum, acetate of lead, in solution, acted very similarly; when weak not preventing coagulation, but preventing it when not much diluted. In the instance of common salt, a solution so weak as to be of the specific gravity 10,045 to water as 10,000, on addition to the vitellus, did not impair its fluidity; it required to be reduced to the specific gravity 10,029 to effect coagulation. The stronger saline solutions, in the same manner as the alkaline, were found capable of dissolving a certain quantity of the coagulated vitellus.

Fourthly, Of the action of acids and some other agents.—The fluid of the vitellus was not coagulated by the tartaric,

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oxalic, or acetic acids, either strong or very much diluted. By strong muriatic acid it was inspissated, the acid and fluid not incorporating. The inspissated mass was transparent; on the addition of water it became opaque and of a milky whiteness, the colour of the ordinary coagulum. The effect of strong sulphuric acid was but little different; whilst the greater portion of the vitellus was inspissated, a very small portion was dissolved, as indicated by its becoming milky on the addition of water, after having been decanted. Nitric acid, whether strong or weak, coagulated the vitellus. A solution of corrosive sublimate had a like effect, as had also alcohol.

The results of these experiments seem to shew that the fluid the subject of them possesses properties distinct from those of either the albumen or yolk of the eggs of birds, or indeed of any other form of albuminous fluid hitherto described; and, in consequence, may it not be held to be a species or variety apart, as much so as the albumen of the serum of blood, or the coagulable lymph of the same fluid?

I could have wished to have extended the inquiry to the ova of the other species of Salmonidæ; but I have not yet had an opportunity, except in an imperfect manner, in the instance of those of the trout and salmon. The results obtained, few as they were, as also on the ova of the pike and perch, were similar, leading to the conclusion, so favoured by analogy, that the ova of all the several species will be found alike in their properties; and further, that the ova, if not of the cartilaginous, at least of the other species of osseous fishes, will not be found dissimilar. But, however probable this may be, it is desirable to have it determined by exact experiments, especially as in the instances of the ova of several of the cartilaginous fishes, comparing one with the other, there are marked differences, both as regards their component parts, and probably as regards also the qualities of those parts: Thus, from such observations as I have made, the eggs of the viviparous fishes of this order appear to be destitute of a white, which those of the oviparous possess. The Torpedo and Squalus squatina may be mentioned as belonging to the former; the Squalus catulus and acanthias, and the Raja oxyrinchus, clavata and aquila to the latter. The yolk of the egg of all these fishes,

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both of those which have and of those which have not a white, seems, in its general properties, to be very similar to that of birds; I can state confidently that it is not coagulated by water. The white (the glairy fluid corresponding in situation to the albumen ovi of birds) will probably be found to possess properties differing from those of the white of the bird's egg. In the instance of that of the Squalus catulus, I found it was neither coagulated by nitric acid nor by heat. In a note, dated Malta, 1832, I have described it "as a transparent viscid fluid, unaltered by boiling during two minutes, in which time the yolk had become hard, and uncoagulated by the addition of nitric acid."

There is a tendency of the mind to seek an object—some end in all that we witness—a final cause—in accordance with the maxim, that Nature does nothing in vain. Reflecting on the property of the ova of the Salmonidæ,—how, so long as they retain their vitality, they remain transparent,—how, on losing their vitality, on the undue admission of water, they become opaque,—it has occurred to me that even this difference may not be without use. The transparent ova are less easily seen than the opaque white, the living than the dead; and, in consequence, the latter may be more attractive, more liable to be preyed on than the former; and the circumstance that the opaque coagulated ova resist change and keep in water a long time, even several months without undergoing any perceptible alteration, is in favour of the conclusion, that they are specially intended for becoming food, serving as lures, and thereby in a manner protecting the transparent, those retaining vitality, and in course of being hatched, from being devoured by birds and fishes.

On the Condition and Prospects of the Aborigines of Australia. By W. Westgarth, Esq.*

1. *Present Aboriginal Population.*

The information under this head is exhibited, for the sake of greater distinctness, in a tabular form. These returns, though in-

* The writer has confined his attention, in the following pages, almost exclusively to the information regarding the Aborigines that has been published within the last two years, which is, in general, of a more practical character

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complete as regards the whole colony of New South Wales, are yet valuable in several respects, as affording some estimate of the ratio of population to extent of country, the proportions of the sexes, and of the children and adults of the aboriginal tribes.

According to Mr Parker's estimate, by a census taken partly in 1843, and partly in 1844, the total number of the Aborigines throughout the district west of the river Goulburn is 1522. This district runs westward to the South Australian frontier, and north from Mount Macedon and Mount William to the Murray. The tribes on the banks of the Murray, still very numerous, are not included. Mr Watton, in the district or country around Mount Rouse, comprising about 20,000 square miles, estimates the number of the Aborigines at 2000.

From the annexed table, it would appear that the proportion of males to females, of all ages, is about 1·56 : 1, or rather more than 3 to 2. The disproportion of the sexes is greater among the children than the adults; the proportion of male to female adults may be estimated at 1·55 : 1, and that of male to female children at 1·8 : 1. The proportion of adults to children is $2\frac{1}{8}$ to 1. That proportion of the territory of New South Wales that may in a general sense be termed "occupied," extends over an area of about 320,000 square miles, and may be estimated to contain above 15,000 aborigines. Allowing 80,000 square miles of this area to Port Philip, and assuming Mr Robinson's estimate of 5000 aborigines, there will be 1 aboriginal inhabitant to each 16 square miles for that district, and 1 to 24 for the remainder of the colony; the average for all New South Wales being 1 aboriginal inhabitant to $21\frac{1}{3}$ square miles.

Considerable numbers of the aborigines were met with by Dr Leichardt and his party on their route to Port Essington, more particularly throughout Northern Australia. The banks of the rivers of the locality appeared comparatively well inhabited, and the travellers encountered native fisheries, numerous wells of fresh water, and the remains of vegetable food prepared for preservation. Captain Sturt gives an interesting account of numerous tribes of the aborigines which he met with towards the central regions of Australia, thickly planted along the grassy banks of a large creek, the bed of which was about the size of that of the Dragging.

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than the observations of preceding writers. The object here proposed being to exhibit the condition and prospects of the Aborigines with reference to their civilisation, or to any degree of benefit that it may be possible to confer upon them, the various and endless *Mythologies* (if they may be so dignified), of the different tribes are very slightly alluded to, and theoretical inquiries as to the primeval origin of the race are not considered.

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District, &c.	Authority.	Adults.			Children.			General Total.	Remarks.
		M.	F.	TOTAL.	M.	F.	TOTAL.		
Darling Downs,	Rolleston	...	...	...	...	...	...	500	
Moreton Bay,	Wickham	...	...	...	...	...	...	4000	There are 300 on Sladbrock Island.
	Simpson	...	...	...	...	...	...	4000	About two females to three males.
Macleay River,	Massie	290	111	401	16	12	28	229	
Port Macquarie,	Cray	210	80	290	...	...	80	370	
Raymond Terrace,	Bench	30	16	46	...	...	7	53	
Dungog,	Ditto	46	14	60	...	...	3	63	From Clarence Town to Underbank.
Scone,	Ditto	...	...	...	...	...	...	75	Between 70 and 80; very few children.
Newcastle,	Ditto	21	12	33	...	...	5	38	In May 1843.
Wollombi and Macdonald River,	Dunlop	44	21	65	5	3	8	73	Consists of three tribes.
Brisbane Water,	Bench	27	13	40	4	3	7	47	
Windsor,	Ditto	40	9	49	...	...	16	65	
Bathurst,	Ditto	...	...	..	..	...	...	150	Sexes stated to be equal in number,
Campbelton,	Ditto	...	...	...	...	...	...	...	None exist as a tribe.
Picton,	Ditto	28	24	52	3	7	15	67	Including 11 half-castes, viz., 2 adults and 9 children.
Berrima,	Ditto	20	15	35	...	...	5	40	The Berrima tribe.
Bungonia,	Ditto	...	...	...	...	...	...	...	There remain only 12 or 15 of the Bungonia tribe, and these have joined some neighbouring tribes.
Goulburn,	Ditto	...	...	...	...	...	..	25	Not exceeding twenty to thirty, of all ages and sexes.
Queanbeyan,	Murray	...	...	...	...	...	...	60	
Mudgee,	Bench	...	...	...	...	...	...	50	One-third males, two-thirds females and children.
New England,	Macdonald	...	...	...	...	...	...	550	Five hundred to six hundred.
Murrumbidgee,	Bingham	1000	400	1400	400	200	600	2000	
Maneroo,	Lambie	311	195	506	...	...	177	683	The children are under twelve years.
Grant County, Port Philip,	Addis	...	...	...	...	...	...	200	Two-thirds males, the rest females with but few children.
Murray District,	Smythe	75	60	135	35	30	65	200*	
Western Port,†	Powlett	350	250	600	250	150	400	1000	
Portland Bay,	Fyans	...	...	...	...	...	...	3000	
Gibb's Land,‡	Tyers	...	...	...	...	...	...	1000	
Port Philip District,	Robinson	1750	1400	3150	...	...	1850	5000	Estimated to be 5000 at least.
River Loddon,§	Parker	248	199	447	...	...	223	670	Thirty-six tribes.
Upper Goulburn and Campaspe	Ditto	107	108	215	...	...	87	302	Nine tribes.
Lower Goulburn and Campaspe	Ditto	...	...	...	...	...	...	200	
Tribes to Westward & Northward,	Ditto	...	...	...	...	...	...	350	
Yarra and Western Port,‖	Thomas	...	...	...	...	...	...	165	Males ninety-nine, females sixty-six.

* Mr Smythe takes the proportions of sexes and ages from those of the aborigines at the crossing place of the Goulburn, where there are 15 men, 12 women, 7 boys, and 6 girls, making a total of 40. The above total of 200 is by mistake made 210 in Mr Smythe's return.

† Comprises all the River Goulburn, the Murray to Swan Hill, south of Mount Cole, the course to the Weirrabee River, Port Philip Bay, and sea coast to Anderson's Inlet.

‡ No accurate estimate of the number of the aborigines can be formed, as they have retreated to parts of the country that are almost inaccessible to the settlers.

§ A large proportion have been personally visited by the Chief Protector, and from one to two thousand have visited the stations.

‖ This is Western Port *proper*, as it may be distinguished from the large Commissioner's District, which from fortuitous circumstances has been comprehended under that name.

Judging from the comparatively numerous aboriginal population in the earlier years of the colony, the present average ratio of aboriginal inhabitants to extent of territory for the entire Australian continent might be anticipated greatly to exceed the very slender estimate above given for New South Wales. But the explorations of Captain Sturt, Mr Eyre, and other travellers, have made known the existence of such extensive tracts of steril country throughout Central and North-west Australia, that it may be doubted if that estimate can be much exceeded.

2. *Their Decrease, and the Causes to which this circumstance is attributable; their Present Condition, and Means of Subsistence.*

The diminution of his number, and the final extinction of savage man, as he makes room for the civilised occupant of his territory, is a feature of which Australia furnishes neither the first nor the only example. The uniform result of all inquiry on the subject of the numbers of the Australian aborigines exhibits a decrease in the population of those districts which have been overspread by colonial enterprise. The ratio of decrease is variously given for different parts of the country. The causes of this gradual extinction appear to be tolerably ascertained; their own mutual wars; their hostile encounters with the whites; the diseases and vices of European society, unusually destructive in their effects, from irregularity in the mode of life, and the want of proper medical treatment; the common practice of infanticide; and, more remotely, perhaps, by the gradual disappearance of various animals used as food, and of other sources of their support. The causes of decrease alluded to by Count Strzelecki are of a striking and important nature. The Australian aborigines do not appear, in general, to want for good humour and contentment; but to one who is accustomed to the comforts of civilised life, their condition, in other respects, appears to have reached the lowest extreme of misery.

The aboriginal *Mahroot* states, that, in his recollection, in Governor Macquarie's time, there were about four hundred individuals of his tribe occupying the southern coast of Port Jackson. There are now but four remaining, namely, three women and himself.

At the Lake Macquarie Mission, the Rev. Mr Threlkeld laboured to acquire the local language, in order to translate the Scriptures, and learn the aborigines of that locality to read; but, in the midst of these efforts, the aborigines themselves, the objects of his exertions, were rapidly disappearing, and, eventually, scarcely any remained to reap the fruits of his zeal.

Assistant Protector Parker estimates the decrease among the tribes of the Loddon and the Goulburn at five per cent. only for the last five years; the Chief Protector's estimate for the entire district for the last six years is twenty per cent. By a census taken at the

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close of 1839, the Yarra and Western Port tribes numbered 207 individuals, who, with five surviving children, subsequently born, make a total of 212. The present number (June 1845), is less by 47, or nearly twenty-three per cent. within the five-and-a-half years.

Mutual war, and hostilities with the whites.—In common with the rest of mankind, in all stages of civilisation, the vicissitudes of aboriginal life are still further diversified by mutual warfare. Mr Robinson estimates that an annual loss of one in twenty of the aborigines is due to this cause, independently of their conflicts with the whites. Ten years ago, observe the Goulburn magistracy, the tribes in that neighbourhood were always at war; they are now, however, much diminished in number, and mingle together as one tribe; and it is necessary that two or three tribes should join together for the performance of a corrobboree.

Out of twenty-one tribes, numbering 421 aborigines, located between the Campaspe river and the west side of the Pyrenees, there occurred twenty-five deaths within a period of two-and-a-half years, ten of which resulted from collisions with aborigines, one with Europeans, the remaining fourteen being due to natural causes. As there were ten surviving children born during this period, the net decrease amounted to fifteen individuals, or about one-and-a-half per cent. per annum. Mr Parker intimates the satisfactory result, that no aboriginal native has been shot within the last three years and a half, though considerable numbers had been thus sacrificed before the establishment of the protectorate. These outrages, on the part of the colonists, are still practised upon the tribes of the Murray, whose territories are situated beyond the influence of the Protectorate. The decrease among these blacks, during the last five years, he estimates at ten to twelve per cent.; and in the district west of the Pyrenees, where many have been killed by the colonists, at the higher proportion of twenty per cent.

The number of blacks who have been killed by the whites throughout the Moreton Bay District cannot be ascertained; but as about fifty whites have already perished at the hands of the aborigines, the destruction has probably been very considerable. Mr Robinson apprehends that the settlers have not scrupled, on occasions, to make use of poison in order to get rid of the aborigines; and Mr Dredge vehemently accuses the former of heartless cruelties towards these unfortunate beings.

Diseases.—In the foremost rank among the miseries that have resulted to the aboriginal population from their intercourse with the whites, must be placed the introduction of that great scourge of the vices of mankind—the venereal disease. Some doubts have, indeed, been expressed in opposition to the general opinion that this disease was originally introduced into Australia by the colonists. The Rev. Mr Schmidt, in reply to a question on this subject from one of

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the Committee, intimated that he found this malady among the Bunya Bunya tribes, some of whom had never been in communication with the whites. He could not, however, form any opinion whether or not these tribes had this disease before or since the arrival of Europeans; nor could the aborigines themselves give any information on the subject. But the agency of the colonists has been terribly effectual in disseminating this disease among these wandering outcasts of the soil. In the various communications to the committee this destructive malady stands prominently forward among the more immediate causes to which the decrease in the numbers of the aborigines is attributable; and its attacks are rendered unusually virulent and distressing, from the exposed and irregular manner of aboriginal life, and the absence of proper medical assistance.* Mr Thomas relates the shocking and frightful extent to which this complaint prevailed throughout the Port Philip district on the arrival of the Protectors. "Old and young," says he, "even children at the breast, were affected with it. I have known hapless infants brought into the world literally rotten with this disease."

Chiefly remarkable amongst the other diseases of the aborigines appears the leucorrhœa, a very prevalent complaint, which rages with great severity. It is a curious circumstance, attested by various experience, that the introduction of this affection among uncivilised races appears to be contemporary with the arrival of European females in the country. It is apt to be mistaken for secondary symptoms, or a modified elephantiasis.†

A great proportion of the aborigines, as stated by the bench of magistrates at Goulburn, have died from pulmonary affections, induced from exposure after intoxication, the effects of which, together with frequent severe rheumatic affections, carry them off in about twelve months after they are attacked. These and other vicissitudes of their mode of life, may be supposed considerably to abridge the usual term of human existence. "One of the men," says Mr Dunlop, speaking of the Wollombi blacks, "aged 55, is blind from old age." Mr Thomas ascertained from returns he has forwarded half-yearly to the government, of the births and deaths of aborigines, that there are at least eight deaths to one birth.

Infant mortality.—The great mortality during infancy is also a remarkable feature among the aborigines. This circumstance is independent of the well-authenticated practice of infanticide, by which

* One of the cures practised by the aborigines for this disease is abstinence from animal food and drinking gum water.

† Strzelecki, p. 347.—The remarks of this writer on the aborigines are always original, forcible, and far-sighted. This is probably the disease alluded to by Dr Lang as having broken out among the aborigines soon after the foundation of the colony. It resembled the smallpox, and rapidly reduced the numbers of the black population, which had been previously very considerable.—(*Lang's History*, second edition, i., p. 36.)

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additional numbers of the helpless offspring are sacrificed to the superstition or barbarism of their parents and tribes. Very few women have more than two children; and the great proportion of the infants do not survive the first month. Of the children born among the Yarra and Western Port tribes during the last six years there is now but one remaining alive. Among the aborigines inhabiting between the river Campaspe and the Pyrenees hills, numbering 421 individuals, the surviving children born during the space of two years and a half were only five males and five females; a much larger number were brought forth, most of whom did not survive a month.

Count Strzelecki has mentioned a remarkable physical law, in connection with the rapid decrease of these aboriginal races, which is but too ominous of their final destiny. It has been ascertained, with reference to various aboriginal tribes, including those of New Zealand, New South Wales, and Van Diemen's Land, that the aboriginal woman, after connection with a European male, "loses the power of conception on a renewal of intercourse with the male of her own race, retaining only that of procreating with the white man."

Condition and means of support.—Their present condition and means of subsistence appear to be well ascertained. In those localities where fish are to be obtained this description of food is in principal use. Mahroot states that his tribe lived generally on fern root, and the fish caught at the sea-coast; the tribe never quitted the sea-coast. The subsistence of the natives about Moreton Bay is derived entirely from the sea. Various roots are also resorted to, particularly that called the *murnong*, a small root of a nutritious character, having a leaf like that of a parsnip, of which they are very fond.

Mr Malcolm thinks that the grazing of sheep and cattle has greatly reduced the growth of this root. Mr Thomas, on the other hand, asserts that it is a mistaken notion that the sheep tend to destroy this root. The native, he says can readily find it out, even without the guidance of the flower. The indigenous roots used by the aborigines are mostly bulbs, very firm in the ground, and, with the exception of pigs, not likely to be destroyed by any animal. The supply of most other descriptions of their food has been either diminished or entirely taken away by the occupation of their country; the kangaroo, for example, and various other animals and birds; and the supply of gum has also been much decreased, in consequence of the extensive exportation of mimosa bark.

The natives must suffer severely in the winter season. The women, with their young infants on their shoulders, may be seen seeking for grubs on mimosa gum; and sometimes, when they are perhaps suckling infants, they will be half a day or night in the water spearing eels. To European minds, the condition of the abo-

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rigines generally suggests the idea of the lowest possible stage of wretchedness.

3. *Infanticide.*

The general prevalence of infanticide is established beyond any reasonable doubt. The half-caste infants appear to be the most exposed to this fate. Among many tribes, they seem to be regularly murdered, either immediately or very soon after birth, unless saved by the interference of the whites. The female infants appear in the next degree exposed to this fate. Occasionally, male and female are despatched alike. According to Mr Lambie, this practice is unknown in Maneroo.

The unnatural coldness on the part of a mother, that might be expected to accompany such a practice, does not appear to exist as a necessary associate; at least, there is on occasions no want of maternal feeling, notwithstanding the apparent inconsistency of such a circumstance. The Moreton Bay blacks have a great affection for their children; but, nevertheless, says Mr Simpson, they eat them when they die from natural causes. If infanticide exists at all, says Mr Dunlop, it must be very rare, and occasioned only by the deepest misery and want. He instances their strong maternal affection.

Of Half-Castes.—It is a rule with the aborigines to destroy their half-caste children immediately after birth, and instances of the kind, at the hands of the mother, Mr Schmidt says, have come under his own notice. On the Manning river, where there are many half-castes, the mothers appear to have a repugnance to them, and several instances are known there, in which they have destroyed these children immediately after birth. On one occasion, a mother, in excuse for destroying her half-caste child, assigned as the reason, that it was half white. Half-caste boys, say the magistrates at Dungog, are believed to be always murdered. Infanticide, says Mr Robinson, exists in Port Philip to a limited extent. The victims have been invariably half-castes; but of late some tribes have spared this class of their offspring. Mr Smythe knows of no half-castes living in his district. Several have been born, but they have invariably disappeared. Mr Parker fears the natives have been hitherto justly charged with the practice of murdering their half-caste children; but a better feeling, he says, now seems to be prevailing, at least among some of the tribes, and he thinks that these children are, in some cases, regarded even with more affection than the pure native. According to Mr Flanagan, the half-castes in the Broulee district, generally disappear about the age of puberty, and are supposed to be destroyed by the other blacks. There are at present about twelve in that locality, and all young.

Of Females.—In New England, where this crime is general, the victims are the half-castes and female infants, never the male. Mr

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Thomas, who considers that infanticide is increasing, states that the blacks were accustomed to destroy the female till a male infant was born; but now he has reason to believe that male and female are alike destroyed. Mr Dredge mentions the practice of murdering all infants of a lighter hue, and the first-born child, if of the female sex.

In general.—In the Broulee district, where infanticide is very common, in the case of twins, one is always sacrificed. Mr Parker states, that the practice appears to have nearly ceased among the Loddon and Goulburn tribes, where the Protectorate influence is felt. No instance, to his knowledge, has occurred among the Loddon tribes during the last two years; but, "to the westward the practice prevails in its grossest and most frightful character. A well-authenticated instance was lately made known to me, in which an infant was killed, and eaten by its mother and her other children." Captain Fyans is convinced that infanticide is a common occurrence, and mentions a case that occurred close to his own residence, where a native man took the child by the legs and dashed its head in pieces against a tree. Mr Thomas speaks despairingly of the prevalence and even increase of the crime. One of the chiefs acknowledged he had no power to stop the practice. The blacks say they have now no country, and are therefore unwilling to keep their children.

4. *Intermixture of Race with the Whites.*

Notwithstanding the squalid aspect of this population, the evidence adduced to the Committee shews a prevalence of illicit intercourse between the aboriginal females and the colonists, chiefly those of the labouring classes. This has been a fruitful source of misery to the aboriginal population, both from the disease that it introduces among them, and from the hostile feeling with which the male blacks of the tribes are justly inspired. There are no instances, the Newcastle Bench states, of the union of whites with the female aborigines, but the labouring classes are in the constant practice of cohabiting with these females, and there appears to be no repugnance on either side.

The number of half-caste children would doubtless have been much greater than it appears to be at present in the colony, but for the well-ascertained practice with many tribes of putting to death all infants of this class. In the Scone District, the majority of the aboriginal children are half-caste, who are living with their mothers. There are many on the Manning river. On Stadbroke Island there are several; in the Picton District eleven, namely, one man, one woman, three male and six female children, who are all living after the manner of the aborigines. Of four half-castes in the district around Brisbane Water, two are adult females, and are married to white men; the other two are children, and living with the aborigines. According to the Chief Protector, there are probably not more than twenty or thirty half-castes in the Port Philip District, who are living with and after the manner of the aborigines.

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5. *Physical Aspect.*

The aborigines of New South Wales and Van Diemen's Land, observes Strzelecki, bear respectively the stamp of different families, together with such variations as the nature of the climate and other conditions of life might impress upon the human frame.

Thus, in New South Wales, where bathing is a luxury, and heat promotes perspiration, the hair is smooth and glossy, the skin fine, and of a uniform colour; whereas in Van Diemen's Land, from the greater coldness of the climate, the skin appears scaly, subject to cutaneous disease, and weather-beaten, and the hair a prey to filthiness.

The facial angle is between 75° and 85°, the forehead low, eyes large and far apart, nose broad and flat, mouth wide, with large white teeth and thick lips, the lower jaw unusually short, and widely expanded anteriorly. The mammæ of the females are not spherical in shape, but pyriform, and soon after marriage they become flaccid and elongated.

The Australian native is adroit and flexible in the motions of his body; in the act of striking or throwing the spear his attitude is extremely graceful. "In his physical appearance, nevertheless, he does not exhibit any features by which his race could be classed or identified with any of the generally known families of mankind."*

The natives of Australia, states Mr Eyre, present a striking resemblance to each other in physical appearance and structure, and in general character, habits, and pursuits.†

6. *Language.*

No feature is more conspicuous among the Australian aborigines than their great diversity of speech; every considerable tribe appearing to have a distinct language of its own. Undoubtedly, the great proportion of these varieties are to be classed as mere dialects, the branches of primary stock, which have deviated more or less widely from their common original, and from one another, according to various accidents in connection with the rarity of intercourse that prevails one with another among the respective sections of the population. But whether or not any of these diversities of speech are traceable respectively to a more remote and independent origin, is a question as yet by no means decided.

Mr Dredge, after alluding to the effect of the separate and dis-

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* Strzelecki, p. 334.

† Paper on the Aborigines of Australia, read before the Ethnological Society. Captain Sturt, during his late hazardous expedition to Central Australia, met with aborigines more tall and more handsomely formed than those of any of the tribes hitherto encountered. Like the aborigines of North Australia, as observed by Dr Leichardt, they made use of food prepared by bruising, and baking seeds.

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tinct character of the respective tribes in varying the language of each, remarks, " that although there are sufficient evidences of the common origin of their language, even tribes separated from each other by comparatively limited spaces, scarcely retain the means of common conversational intercourse." He instances one curious custom or superstition, prevalent amongst some of the aboriginal population, the continuance of which throughout successive ages, must at length introduce extensive diversities into the language of each of the separate tribes. This is the practice of never again uttering the names of individuals of the tribe after their decease, especially in cases where death has occurred through violence. On one occasion, an individual of a tribe, whose name was the term for fire, was murdered by one of a different tribe; and, in accordance with the usage just alluded to, the word representing fire was thenceforth discontinued, and a new term created. It is easy to conceive that such alterations might occur frequently.*

Count Strzelecki is of this opinion, however, that there has been too much haste and eagerness in deciding on the affinities of the languages of the various tribes, and referring them all to one common root. The three natives who accompanied Captain Flinders and Captain King, and those who accompanied himself, were unable to understand one word spoken by the tribes of other districts.†

7. *Religious and Social Institutions, Customs, and Manners.*

Religious Ideas.—The nature of the religion and government of the Australian aborigines, remarks Count Strzelecki, is still involved in mystery. They certainly recognise a God, whom they call " Great Master," regarding themselves as his slaves; and hence, probably, they entertain no feeling of obligation or gratitude for the gift of life, or their other enjoyments, considering that it is the Great Master's duty to supply them with these. They believe in a future immortality of happiness, and place their heaven in the locality of the stars. They do not dread the Deity. Their fears are reserved for the evil spirit, who counteracts the work of the Great Master, and consequently the former is the object to whom their worship is directed.

According to Mr Eyre, the natives of the Murray entertain the belief that there are four individuals called Nooreele, who live among

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* Dredge, p. 7.

† Strzelecki, p. 337.—Mr Hull brings forward some curious coincidences of sounds and meanings in aboriginal Australian words with those of several languages, ancient and modern, of the northern hemisphere. But these fortuitous or isolated facts can lead to no definite results; unless, indeed, to shew that some branch of the Australian tongue may approach, in the possibility of accidents, more nearly to Greek or Latin, than to the ever-changing dialects of its own stock.—(*Remarks*, &c. p. 7.)

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the clouds and never die. Of these superior powers, the Father, who is omnipotent, and of a benevolent character, created the earth and its various objects. The Nooreele are joined by the souls (literally *shadows*) of men after death, and they are thenceforth immortal.*

Social Institutions.—Strzelecki observes there are three social gradations or classes among the aborigines. These successive steps are attained through age and fidelity to the tribe. The highest class, consisting commonly of the aged few, is the only one that is initiated into the religious mysteries, and the regulation of the affairs of the tribe. The meetings of this class are of a sacred and secluded character. On one of these occasions, he himself was warned off from the vicinity, and could not, without personal danger, have approached within ten miles of the meeting.

The aborigines are divided into a number of tribes, some much more numerous than others, but the greatest of them seldom consisting of more than two or three hundred individuals. But these tribes, whether large or small, weak or powerful, are always perfectly distinct, separate from and independent of one another, each inhabiting a tract of country of its own. The general control and management of their affairs appears to be, by mutual consent, in the hands of the adult males respectively of each tribe.

Manners and Customs.—The result of this exclusive feeling is a narrowness of mind, arising from inexperience and want of information. Each tribe denominates as "wild black fellows" all others who are beyond the limits of its acquaintance. Every stranger who presents himself uninvited among them, incurs the penalty of death. This sanguinary custom is traceable to a superstitious belief that the death of any member of a tribe is occasioned by the hand of some enemy, who has come upon him unawares; and hence any stranger found in the camp is suspected of being upon this hostile mission. So general is this exclusive and hostile feeling, says Mr Thomas, that measures should be adopted to prevent any parties from taking blacks out of their own districts.

This belief or superstition has originated the practice, on the occasion of a death in the tribe, of sacrificing some individual of a neighbouring tribe, who is supposed to be the murderer. The plan

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* The description given by the aborigines of their religious ideas appear vague and undefined, and different among the separate tribes. In pursuing inquiries on this subject, there must be great difficulty on both sides in comprehending the precise nature, both of the questions and the answers. The caves and paintings discovered by Captain Grey are a curious circumstance in the religious indications of the aborigines, and betoken more of system and reflection in their minds than might be expected from their appearance and general characteristics.—(See Mr Hull's "*Remarks*," p. 28, where sketches of the paintings are given.)

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adopted for the discovery of the supposed criminal, is to watch the course taken by any insect near the body, and to follow their prey in that particular direction.*

Count Strzelecki confirms this statement, in an interesting account he gives of his rencontre on one occasion with a tribe of aborigines in Gipps Land. The tribe was seen encamped around a pond; and as the traveller had been several days without water, he would have instantly rushed forward to quench his burning thirst. But his guide earnestly prevented him, and they sat down near the encampment. After an interval of a quarter of an hour, a piece of burning wood was thrown to them, with which they lighted their fire, and proceeded to cook an opposum they had in store. The guide then began gnawing the stick, occasionally stirring the fire, at times casting his looks sideways. Presently a calabash of water was brought them. After appeasing hunger and thirst, the traveller was about to close his weary eyes, when an old man came out from the camp. The guide met him half way, and a parley ensued as to the object of the Count's wandering. The old man having returned with the answer, a thrilling and piercing voice was next heard relating the subject to the tribe. Silence ensued for a few moments, after which the travellers were ordered to return whence they came. There was no appeal.

Connected with these wary and distrustful feelings of the aborigines is, perhaps, to be considered the strong repugnance they manifest to revisiting a spot where one of their tribe has happened to die. At the German mission, after many abortive attempts, several natives were at length induced to clear some ground and erect slab huts for their own residence. A few weeks afterwards, however, a death occurred amongst the group, which caused the huts to be deserted, nor could any entreaty, or the inclemency of the weather, tempt them to return.

The mode of disposing of the dead varies according to the usage

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* Smythe, 2. Similar information was given to the writer several years ago, regarding the natives of the Colac district. The occurrence of a death, even though from accident or natural causes, is attributed to some party of a neighbouring tribe, who has secretly abstracted the kidney fat of his supposed victim, this being a favourite morsel among the blacks, and frequently plucked out and devoured from the living bodies of their enemies. Their manner of proceeding is to bury the body in the ground, carefully smoothing the surface, so that it may exhibit the direction taken by any animal or living creature over the grave. The tribe immediately starts off in the direction first indicated, and the first strange native who is met with becomes the victim. It is not perhaps to be wondered at, that, under the influence of superstition, which exerts such powerful and inexplicable effects even upon civilised man, the fact of the entire outward aspect of the body of the comrade thus avenged, and the actual presence of the untouched fat itself, should not in any wise affect the case. The Colac tribes are now much reduced in number; and the thickly planted pastoral settlements of that romantic and beautiful country, have probably had the effect of blunting the edge of their zest for these senseless barbarities.

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of the district and the age of the deceased. One process is by simple burial; another, the burning of the body; a third, drying the body in the sun. The lamentations for the dead are frequently prolonged beyond the time of burial, and the cries of the women may be heard by the traveller during the midnight hours, as they issue with strange and varied effect from the lonely woods.*

Amongst these wandering tribes, it is curious to find that the rite of circumcision is practised, and, to all appearance, very generally, throughout Australia. Dr Leichardt, in his Journal, mentions that all the aboriginal tribes that were met with by his party around the Gulf of Carpentaria, practised this rite. It is also practised by the aborigines of the Colony of South Australia, which is situated at the opposite part of the country.† Cannibalism does not appear to prevail extensively throughout Australia; it exists in some of the tribes.‡

8. *General Character, and Degree of Aptitude for Employment and Civilisation.*

The qualities and capabilities of the aboriginal mind are the subject of considerable diversity of opinion. By those who have most experienced its workings, the aptitude for civilised life, and the perception of moral obligations are in general portrayed in very discouraging colours. There is, indeed, with the aborigines, a facility of imitation of European manners and habits, united to a simplicity and docility of character, arising actually from a prostration of spirit and quiescence of the higher departments of the mind, that are ever apt to give favourable impressions to an ardent disposition.§ The most tractable and the most promising, wearied out, after a period, by the monotonous avocations of civilised life, or drawn aside from a course of apparent well-doing by some ancestral custom or superstri-

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* The Port-Philip aborigines plaster the face and hair of the head with white clay, when mourning for the death of a member of the family.

† Mr Hull's "Remarks on the Probable Origin and Antiquities of the Aborigines," (just published) page 16, where he describes the manner of performing the operation.

‡ The aborigines of the southern parts of Australia are said to make use of human skulls as drinking vessels,—a statement, however, which the writer has not heard properly confirmed. Every *gin* or wife, it is stated, possesses this description of calabash, which she usually fabricates herself; and the aborigines appear to have practised the art of fashioning these vessels from time immemorial. According to Professor Owen, this is the first instance of the habitual conversion of a part of the human skeleton to a drinking vessel.

§ Yet Mr Eyre describes the character of the Australian as frank, open, and confiding, and, when once on terms of intimacy, marked by a freedom and fearlessness that by no means countenance the impression so generally entertained of his treachery. The apparent inconsistency here is in expecting from the native the same rules of thought and motives of action that prevail with civilised man, and regarding as treachery that conduct which is simply the result of a radically unchanged mind and habits.

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tious usage, some temptation of uncontrolled appetite, or strong appeal to the instinctive workings of an unreflecting mind, may suddenly throw aside the loose and cumbrous mantle of civilisation, and return with unabated zest to his native woods and his original barbarism.

Degree of aptitude for the employments of Civilised life.—In a country like New South Wales, where there is generally a great demand for labouring population, the most favourable opportunities constantly offer for introducing the aborigines within the pale of civilisation, and enrolling them in the ranks of the labouring community of the country. But all attempts, to effect this object have, generally speaking, proved a failure. Accustomed to habits and pursuits and ideas altogether different, those exhibited by Europeans appear to them incomprehensible, and they cannot be induced to remain steadily at any particular occupation. They soon exhibit symptoms of impatience, and a sensation of irksomeness under the monotony of ordinary daily labour. Although they seem as intelligent, comparatively speaking, as the working people around them, speak English in some instances remarkably well, have a full knowledge of the value of money, and are quite competent to form notions of the comforts of civilised life, yet they appear totally indifferent to these attractions, and prefer their own misery and wretchedness.

But amidst the thousand varieties of employment useful and necessary to society, it is not to be expected, but that even the wildest passions and the most unruly habits may find some fitting sphere of congenial activity. A number of the aborigines have been formed into a body of "Native Police," for the protection of the interior districts, and appear to have even exceeded expectation in this capacity. According to Mr Powlett, about forty natives of the tribe south of the Yarra, are employed in this police force. They are of great utility to travellers, from "their knowledge of locality, quickness of perception, endurance of fatigue, and their facility in procuring water and sustenance." The Messrs M'Arthur employ two aborigines as shepherds, who receive the usual wages of that class; and according to Mr Powlett, about fifteen or twenty are similarly employed in his district, who are remunerated by supplies of rations and clothing. The Berrima tribes, during harvest time, are generally employed in reaping, which they perform very well, and are remunerated partly in money and partly in clothing, and tea, sugar, and tobacco. But though active enough for a while, and indeed frequently the best labourers in the field, they are not enduring. Only a few can be induced to work at a time, and these but for a short period. When fatigued, they will not work for any consideration. The Rev. Mr Schmidt, who also notices their want of steadiness, though quite able to perform all kinds of manual labour without difficulty, remarks that from five to seven weeks, at one

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time is the longest period he has known natives to continue at work in one place.

Though legislative enactments may do little, observes Mr Rolleston, yet much may be accomplished individually with the aborigines; and he instances his own black servant, whom he finds more serviceable in every respect than a white man.

Moral character.—The Rev. Mr Schmidt feelingly describes the want of gratitude in the aboriginal mind. At the Missionary station, notwithstanding every kindness, the natives would steal all they could get at. Those on whom the missionaries had bestowed the greatest attention, appeared to have turned out the worst of all, and were in reality the ringleaders in mischief and wickedness. One of them speared one of the missionaries, who narrowly escaped being roasted and devoured. They have occasioned great destruction of property at some of the stations, independently of what they consumed. "In fact, they have, although they have been fed, and received wages at our station, attacked and plundered the gardens, and taken away whatever they could." Mr Massie instances a hut-keeper* who was invariably kind to the aborigines, but whom they treacherously and barbarously murdered.†

"The female aborigines," remarks Mr Dunlop, who appears to have considered the subject with the warm interest and the inspiring hopes of a religious mind, "are as modest in demeanour, and quite as morally conducted as the native, or otherwise free women. There is no instance of their leaving their tribe, or connecting themselves with the white labouring population."

Aptitude for instruction.—Testimony has been repeatedly furnished that there is no general defect or incapacity in the aboriginal mind with regard to memory, quickness of perception, or even the acquirement of the usual elements of education. This is abundantly exemplified in the success of the present experimental school for aboriginal children at the Meri Meri Creek, under the direction of Mr Peacock. This quickness of the aboriginal children is alluded to by Mr Dredge, in regard to the facility with which they learn to read; and he further remarks the readiness with which the young men take up various branches of pastoral labour. Mr Massie states that a young half-caste boy he has in charge, is rapidly advancing in his education, and exhibits even greater aptitude for learning than is generally met with in a white boy of his own age.

Mental capacity.—But the symptoms are more doubtful with regard to the higher mental indications. Apt in many departments of

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* The servant at the squatting out-stations, who acts as cook, &c., is usually so called, in contradistinction to those who go forth daily with the sheep.

† With characteristics of this description, it is rather amusing to understand that they entertain an insuperable objection to wearing any slop clothing that resembles the convict dress.—Dunlop, 12.

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knowledge, minutely observant of transactions, often amazingly shrewd and intelligent, the untutored savage shines with a lustre of his own, which appears in some respects as much superior, as in others it is manifestly inferior in the comparison with the civilised man. The casual observer is perplexed by seeming inconsistencies. But it is here that these two classes of mankind most widely diverge.

In answer to a question from the Committee on this subject, the Rev. Mr Schmidt admitted that any high degree of intelligence cannot be communicated to any black in one generation. He regards the aboriginal Australian as the lowest in the scale of the human race that has come under his notice. "They have no idea of a Divine Being; the impressions which we sometimes thought we had made upon them prove quite transient. Their faculties, especially their memories, are in some respects very good; but they appear to have no understanding of things they commit to memory—I mean connected with religion." There is, he continues, either something wanting in their minds that occasions this defect of understanding upon abstract matters, "or it is slumbering so deeply, that nothing but Divine power can awaken it." The testimony of Mr Parker is to a similar effect. The conveyance of truth, says he, to the mind of an Australian savage is attended with formidable, he might almost say insuperable, difficulties. "What can be done with a people whose language knows no such terms as justice, sin, guilt, &c.; and to whose minds the ideas conveyed by such are utterly foreign and inexplicable."

(*To be concluded in next Number.*)

On the Geysers of California.

Professor Forest Shepherd, in a communication published in Silliman's Journal, September 1851, gives an account of some remarkable geysers discovered by him north-west of the Napa Valley, California. Mr Shepherd having noticed, what he conceived to be, a line of thermal action in the Napa Valley, especially near the foot of Mount St Helena, determined to trace it, and find its seat or focus of greatest intensity. With this object in view, he travelled, in company with a select party, in a direction north-west of the Napa Valley, and after encamping one or two nights in the rain, and wandering through almost impenetrable thickets, reached the summit of a peak on the morning of the fourth day. The scene presented from this point is described as follows:—

"On the north, almost immediately at our feet, there opened

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an immense chasm, apparently formed by the rending of the mountains in a direction from west to east. The sun's rays had already penetrated into the narrow valley, and so lighted up the deep defile, that, from a distance of four or five miles, we distinctly saw clouds and dense columns of steam rapidly rising from the banks of the little river Pluton. It was now the 8th of February, the mountain peaks in the distance were covered with snow, while the valley at our feet wore the verdant garb of summer. It was with difficulty we could persuade ourselves that we were not looking down upon some manufacturing city, until, by a tortuous descent, we arrived at the spot where at once the secrets of the inner world opened upon our astonished senses. In the space of half a mile square we discovered from one to two hundred openings, through which the steam issued with violence, sending up columns of dense vapour to the height of one hundred and fifty to two hundred feet. The roar of the largest tubes would be heard for a mile or more, and the sharp hissing of the smaller ones is still ringing in my ears. Many of them would work spasmodically, precisely like high-pressure engines, throwing out occasional jets of steam, or volumes of hot scalding water, some twenty or thirty feet, endangering the lives of those who rashly ventured too near. In some places the steam and water come in contact so as to produce a constant '*jet d'eau*' or spouting fountain, with a dense cloud above the spray, affording vivid prismatic hues in the sunshine. Numerous cones are formed by the accumulation of various mineral salts and a deposit of sulphur crystals with earthy matter, which often harden into crusts of greater or less strength and thickness. Frequently the streams of boiling water would mount up to the top of the cones with violent ebullition. Some of the cones appear to be immense boiling caldrons, and you hear the lashing and foaming gyrations beneath your feet as you approach them. It is then a moment of intense interest—curiosity impels you forward—fear holds you back; and while you hesitate the thin crust under your feet gives way, and you find yourself sinking into the fiery maelstrom below. The writer, on one occasion, heard the rushing water under his feet. He struck

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down an axe, which, on the first blow, went through into the deep whirlpool the whole length of the helve. He withdrew it and cut an opening, which revealed a stream of angry water, boiling intensely, and of unknown breadth and depth. He continued to enlarge the opening until the stream was seen to be five or six feet in breadth, leading on indefinitely into the dark caverns beneath the mountains.

"At the base of the cones, in the bottom of the ravines, and in the bed and on the north bank of the river Pluton, springs almost innumerable break out, which are of various qualities and temperatures, from icy coldness up to the boiling point. You may here find sulphur water precisely similar to the celebrated *white sulphur* of Green Brier County, Va., except its icy coldness; also red, blue, and even black sulphur water, both cold and hot; also pure limpid hot water without any sulphur or chlorine salts, calcareous hot waters, magnesian chalybeate, &c., in an almost endless variety. Where the heated sulphuretted hydrogen gas is evolved, water appears to be suddenly formed, beautiful crystals of sulphur deposited, (not sublimed as by fire) and more or less sulphuric acid generated. In some places the acid was found so strong as to turn black kid gloves almost immediately to a deep red. Where the heated gas escapes in the river Pluton, such is the amount of sulphur deposited, that the whole bed of the stream is made white for one or two miles below. Notwithstanding that the rocks and earth in many places are so hot as to burn your feet through the soles of your boots, there is yet no appearance of a volcano in this extraordinary spot. Were the action to cease, it would be difficult, after a few years, to persuade men that it ever existed. There is no appearance of lava. You find yourself not in a solfatara, nor one of the salses, described by Humboldt. The rocks around you are rapidly dissolving under the powerful metamorphic action going on. Porphyry and jasper are transformed into a kind of potter's clay. Pseudotrappean and magnesian rocks are consumed much like wood in a slow fire, and go to form sulphate of magnesia and other products. Granite is rendered so soft that you may crush it between your fingers, and cut it as easily as bread unbaked. The

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feldspar appears to be converted partly into alum. In the mean time the boulders and angular fragments brought down the ravines and river by the floods are being cemented into a firm conglomerate, so that it is difficult to dislodge even a small pebble, the pebble itself sometimes breaking before the cementation yields.

"The thermal action on wood in this place is also highly interesting. In one mound I discovered the stump of a large tree, silicified; in another, a log changed to lignite or brown coal. Other fragments appeared midway between petrifaction and carbonization. In this connection, finding some drops of a very dense fluid, and also highly refractive, I was led to believe that pure carbon might, under such circumstances, crystallise and form the diamond. Unfortunately for me, however, I lost the precious drop in attempting to secure it.

"A green tree cut down and obliquely inserted in one of the conical mounds, was so changed in thirty-six hours, that its species would not have been recognised, except from the portion projecting outside, around which beautiful crystals of sulphur had already formed.

"From the thermal exhalations and the amount of sulphur deposited, it might be supposed that the progress of vegetation would be retarded; but such is not the fact, on the contrary, it is greatly facilitated. The *Quercus sempervirens*, or evergreen oak, flourishes in beauty within fifty feet of the boiling and angry geysers. Maples and alders, from one to two feet in diameter, grow within twenty or thirty feet of the hottest steam pipes. This, however, may be accounted for by the cold surface water flowing down from the adjacent mountains. Multitudes of grizzly bears make their beds on the warm grounds. Panthers, deer, hares, and squirrels, also take up their winter quarters in the very midst of the geyser mounds. Farther down the stream, on the terraced banks of the limpid Pluton, vegetation *actually runs wild;* and the winter months exhibit all the fancied freshness of primeval Eden. I have traced the influence of this thermal action from two to three hundred miles on the Pacific coast in California, but only in this place have I been permitted to witness

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its astonishing intensity. The metamorphic action going on is, at this moment, effecting important changes in the structure and conformation of the rocky strata. It is not stationary, but apparently moving slowly eastward in the Pluton Valley." —(*American Annual of Scientific Discovery*, for 1852.)

On Meteorites. By CHARLES UPHAM SHEPARD, M.D., Professor of Chemistry and Mineralogy. Communicated by the Author.*

1. *Tuttehpore, Hindostan, Nov.* 30, 1822.

This stone, so far as I am informed, has not been described. It is barely mentioned by Prof. Partsch, in the Appendix, p. 142, of his Catalogue of Meteorites in the Imperial Collection at Vienna (1843), as not yet brought into Europe. While in Edinburgh last year, I was informed by Mr Alexander Rose, that a fine specimen of this locality existed in the cabinet of Thomas M'Pherson Grant, Esq., by whom I was very obligingly presented with a fragment, and the means of making the present communication.

The fall took place in the evening at Tuttehpore, which is situated seventy-two miles from Allahabad, on the Cawnpore road, in lat. 25° 57′ N., and long. 80° 50′ E. The meteor from which the stone was ejected, was of large size, surpassing the full moon in apparent magnitude as well as splendour. It passed from south-east to north-west. A number of stones fell, the largest of which weighed 22 lb., but that in the possession of Mr Grant was the only one in an entire state which was found. It was brought from India by Dr Tytler, by whom it was presented to its present owner.

The stone is oval, slightly compressed, indented, and possesses a brownish-black crust. Its weight is about 2 lb. It is fine-grained, trachytic, and resembles most closely the stones of Poltawa (March 12, 1811), and of Castine (May 20, 1848.) Sp. gr. = 3·352.

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* Read before the American Association for the Advancement of Science, at New Haven, August 1850.

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2. *Charwallas, 30 miles from Hissar, India, June* 12, 1834.

This is another stone of which the only notice I have met with is found in the Appendix of the above-mentioned work (p. 143), Prof. Partsch remarking that no portion of the mass had made its way into Europe. The entire stone is in the possession of Prof. Jameson, to whom it had been presented —for the Museum of Natural History in the University of Edinburgh—by a gentleman resident in India at the time of its fall. Its exact weight I am not able to give; but I have the impression that it cannot fall short of 7 lb. or 8 lb. I owe a slice of it to the kindness of Professor Jameson, from an examination of which I am able to give the following description.

It is one of the toughest stones, if we except those of Chantonnay (Aug. 5, 1812) and Cabbarras Co., N.C. (Oct. 31, 1849), with which I am acquainted. It is filled with iron rust, like certain weathered, fine granular granites, in consequence of which, and the smallness of the particles of composition, it is impossible to recognise the mineral species (with the exception of the nickeliferous iron) of which it is made up, although olivinoid, and one of the feldspar species, appear to be the leading ingredients.

On exposure to the air, it deliquesces, yielding chloride of iron; but this does not prove chlorine to have been an original ingredient of the stone, since the mass, as in the case of one of the Iowa (Feb. 25, 1847) stones, may have been since its fall in some situation where chlorine has been imparted to it.

Its specific gravity is 3·38. It contains 15·07 per cent. of nickeliferous iron, with traces of sulphur. The stony part consists of silica, magnesia, protoxide of iron, alumina, and lime.

3. *Meteoric Iron, County Down, Ireland. Fell August* 10, 5 P.M., 1846.

For a knowledge of this meteorite I am indebted to my friend Dr John Scouler, Prof. of the Royal Dublin Institution, who wrote me as follows, respecting its fall, in February 1848. "I believe I must give you the credit of having discovered another meteorite in Ireland, or in other words, but for you I would not have been at the pains of finding it out. The stone or stones fell in 1844, in the north of the county

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of Down, and were seen to fall by some of the coast-guard. You will find two small specimens of this stone along with the other specimens in the box." Owing to an accident in the transmission of the box, the specimens were not received until within a few months, and hence the delay in making known this interesting fall of meteoric iron. The only additional information concerning the event, which I am at present able to communicate, is the circumstance mentioned in the label accompanying the specimens, "that the name of the man who saw the mass fall, and who picked it up, was Gibbon."

The following is all that I am able at present to make known concerning the mass. It is malleable, homogeneous, and amygdaloidal. Specific gravity variable; vesicular portions = 5·9. Crust thick, sometimes one-third of an inch, and consists of mixed oxides of iron, somewhat coated by blue phosphate of iron (vivianite). In moist air, the chloride of iron deliquesces in little drops. It does not afford the Widmanstättian figures. It does not contain nickel, cobalt, or sulphur.

4. *Description of a Large Stone of the Linn Co., Iowa, fall of Feb.* 25, 1847.

This stone, weighing 20 lb., has lately come into my hands through the agency of Rev. R. Gaylord, of Hartford, Iowa,—the same gentleman who procured for me the specimens which were picked up at the time of the explosion of the meteor, and of which an account was given at a former meeting of the Association. (See vol. iv., 288, 289, of Silliman's Journal.)

The following statement respecting it is from the Rev. Mr Gaylord's letter of July 3, 1850. "It was found (in the summer of 1847) in Hooshier Grove, by Abner Cox. He was in company with John Hollis, of whom I obtained two fragments three years ago. They have had the stone two years or more and by lying in the loft of a smoky cabin, it is somewhat dingy in appearance. This John Hollis is the man who ground up so much of the stones that were seen to fall, in order to get silver. He was the means, however, of the careful preservation of the present mass. Dr Knight found they had the stone, and wrote me respecting it.

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"The three pieces into which it broke on striking the ground fit together exactly, so as to reproduce the original stone, with a complete coating over the whole, except on one side, where several small fragments were broken out by the fall. These were gathered up carefully, and preserved by the finder."

This stone is perhaps the most remarkable one thus far described, for its highly regular prismatic figure, which at once suggests the idea of a portion of a basaltic column. Nor can the geologist look upon it without feeling almost certain that it once formed part of some extensive formation in the world from whence it came.

5. *Meteoric Stone of Waterloo, Seneca Co., N. Y.; fell in the summer of* 1826 *or* 1827.

For my first knowledge of this remarkable stone, I am indebted to Prof. O. Root of Hamilton College, from whose letter, dated Clinton, N. Y., Jan 26, 1850, the following abstracts are made. "On receiving your note, I wrote to my friends in Geneva, for the meteorite mentioned in my letter to President Hitchcock. Judge Watkins very willingly gave the specimen, and it is now in my possession, subject to your order. The piece is not large (it weighs about 1000 grs.), as the original mass had been divided two or three times. Not being familiar with such productions, my opinion concerning its genuineness is of no value. Judge Watkins, however, is a gentleman of high respectability, and I have confidence in what he relates of the history of this stone. My attention was directed to the subject in the following manner: A year or two ago, while shewing some gentlemen a fragment of the Otsego meteoric iron, one of them observed that he remembered a report many years back of a stone falling through a roof in Waterloo, or in that vicinity. After many inquiries, I at last found the stone, or a fragment of it, with Judge Watkins. He relates that a hole was discovered in the roof of his mill, directly over a bin of wheat, that the opening was made through the shingles where the roof-boards were about five inches apart (although a piece was split from the roof-board on one side), and that

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under the hole there appeared a depression in the grain, which led to an examination that resulted in the discovery of the stone. The Judge inferred that the stone had fallen through the roof, as its size was too great to have allowed its admission into the bin along with the grain, which was raised by means of elevators. He also supposed it to have been of atmospheric origin, as the mill was four storeys high and as the nature of the stone was unlike any of the mineral productions of the region, the rock *in situ* at Waterloo being the Seneca limestone. He was not positive whether it was found in 1826 or 1827. The stone was divided for Dr Hale, President of Geneva College."*

The specimen presented me by Prof. Root had been left for upwards of twenty years in the garret of Judge Watkins, where it appears to have been mistaken for something edible by the rats, who have left numerous markings of their incisor teeth upon its surface. Indeed, in colour and texture, it nearly resembles common rhubarb. Its colour is light buff or yellow. It is slightly coherent, and may easily be crushed between the fingers. Its sp. gr. = 2·30. But a small portion of the original crust remains, which is reddish-brown. The stone contains in small quantity, blackish particles attracted by the magnet. A surface produced by being cut with a saw, shews waved parallel lines of greater hardness than the rest of the stone. It consists of

Silica, ...	78·80
Peroxide of iron, ...	8·72
Alumina, ...	6·28
Moisture, ...	4·75
	98·55
Lime and magnesia (in equal quantities), and loss,	1·45
	100·00

6. *Specific gravities of two meteoric irons.*

Meteoric iron of Pittsburg, Pa., . .	7·380
Meteoric iron of Salt River, Ky., .	6·835

* I addressed a letter of inquiry to Dr H., who informs me that the specimen has for some time been lost sight of in the College collection.

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Chemical Examination of Drift-Weed Kelp from Orkney. By GEORGE W. BROWN, Esq. of Glasgow. Communicated by the Author, through Dr R. D. THOMSON, and read before the Philosophical Society of Glasgow.*

Drift-weed kelp is derived from the sea-weeds which grow on the rocks at the bottom of the Atlantic Ocean. These plants being torn from their native soils by the force of tides and currents, are drifted to the north and north-west coasts of Scotland and Ireland, on which they are thrown by the surge, and being gathered are burnt either in kilns or in depressions dug in the ground.† By this process most of the organic matter is removed, although in the specimen of kelp investigated and described in this paper, a small portion of carbon and nitrogen still remained. The most important constituents of kelp are the iodine and potash salts. The carbonates were formerly used by the soapmakers, and the insoluble salts for the manufacture of bottle-glass.

Previous Analyses.—Although the composition of the kelp salts is well known, in a general point of view, to the professional chemist, it does not appear, from any experiments which have been recorded, that they have been made the subject of recent minute investigation. Mr Kirwan, in the end of the last century, published a paper, (Memoir read at the Royal Dublin Society, and Annales de Chimie 1793, tom. 18, page 163), *On the Alkaline Substances employed in Bleaching Linen.* The following is his analysis of what he calls sweet barilla from Spain, which corresponds with kelp in its physical characters.

Carbonic acid, ...	16·66
Carbon, ...	14·95
Lime, ...	9·42
Magnesia, ...	2·20

* The author conducted the "Examination," under the immediate superintendence of Dr Thomson.

† History and Description of the Kelp Manufactory. Proceedings of Glasgow Philosophical Society, vol. ii, page 241. By Mr Glassford.

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Clay,	2·27
Silica,	4·33
Soda pure,	14·63
Soda impure,	4·34
Soda with common salt,	2·20
Sulphate of soda,	2·17
Chloride of sodium,	1·21
Earthy deposit,	·34
Water,	25·23

In another volume of the Annales de Chimie there is a paper by M. Gay-Lussac, (Annales de Chimie for 1828, tom. 39, pp. 159–163), on "The Potashes of Commerce," in which he has an analysis of the salts of Wareck, which is as follows:—

Sulphate of potash,	22·2
Chloride of potassium,	24·6
Chloride of sodium,	53·2

This appears to be a general statement of the constitution of the soluble salts only. Dr Ure, in his Dictionary of the Arts and Manufactures, published in 1840, gives the extremes of his analyses of kelp as follow:—

Soluble Salts.	Sulphate of soda,	8·0	19·0
	Carbonate of soda and Sulphuret of sodium,	8·5	5·5
	Muriates of potash and soda,	36.5	37·5
	Carbonate of lime,	24·0	10·0
	Silica,	8·0	0·0
	Alumina, with oxide of iron,	9·0	10·0
	Sulphate of lime,	0·0	9·5
	Sulphur and loss,	6·0	8·5
		100·	100.

The peculiarity of those results is in the presence of a large quantity of alumina, and the absence of phosphate of lime and alkaline phosphates, which are at variance with the analyses to be described in this paper.*

There are analyses of the ashes of some kinds of fuci, viz.,

* The quantity of phosphate of lime analyses corresponds with what Dr Ure terms alumina. It would be difficult to explain the source of such an amount of this earth, as alumina rarely or never enters into the constitution of the vegetable kingdom.

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F. digitatus, F. vesiculosus, F. nodosus, and F. serratus, in Liebig's Annalen, vol. liv., p. 350, by Mr J. Gödechens of Hamburg, published in 1845. The following table gives the calculated mean of these analyses :—

Potash,	11·67
Soda,	12·54
Lime,	11·32
Magnesia,	8·29
Oxide of iron,	0·32
Chloride of sodium,	20·61
Iodide of sodium,	1·33
Sulphuric acid,	19·77
Phosphoric acid,	2·19
Silica,	1·01
Carbonic acid,	5·27
Carbon,	6·05
	100·37

Analysis of Orkney Drift-Weed Kelp.

For the specimen of kelp subjected to examination I am indebted to W. Paterson, Esq., alkali-merchant, Glasgow. The investigation was conducted in the laboratory of the University of Glasgow, under the superintendence of Dr R. D. Thomson. In making these analyses the first points to be determined were, the quantity of soluble and insoluble salts and water. To effect the first object, a portion of the kelp was digested in water, the solution and residue thrown upon a weighed filter and washed till all the soluble salts were removed. The insoluble salts were then dried at 212° F :—

	Insoluble Salts.	Soluble Salts and Water.	Insoluble Salts per cent.	Soluble Salts and Water per cent.
400 grains gave	114·80	285·20	28·700	71·3
500 ...	155·49	344·51	31·098	68·902
1000 ...	294·10	705·90	29·410	70·59
		Mean,	29·736	70·264

Water.—The quantity of water was estimated by heating the kelp at 212° F. till it ceased to loose weight :—

	Water.	Water per cent.
200 grains gave	13·60	6·8

If the quantity of water be subtracted from the soluble

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salts and water, the real amount of soluble salts will be obtained, which is as follows :—

Insoluble Salts.	Soluble Salts.	Water.
29·736	63·464	6·8

Analysis of Insoluble Salts.

The following is the description of analyses and results obtained from the insoluble salts.

Testing Analysis of Insoluble Salts.—Before proceeding to the quantitative analysis of the insoluble salts, a qualitative investigation was made. The kelp under examination was very hard, with a strong alkaline taste, and greyish colour, with black portions of carbonaceous matter interspersed through it. It was partly soluble in water. That which remained undissolved in water was a greyish powder, which, when ignited, became perfectly white. On addition of acid to the insoluble matter, carbonic acid and sulphuretted hydrogen were evolved, and the greater part of the salts dissolved, that which remained being silica. The portion thus dissolved in acid gave, on addition of ammonia, a copious precipitate, which proved on examination to be phosphate of lime. To prove the presence of phosphoric acid it was converted into phosphate of iron by Berthier's method, and the phosphoric acid precipitated, as ammonia phosphate of magnesia, the iron being detained by tartaric acid, with a trace of iron. On the phosphate of lime being separated by filtration, the filtrate gave, with oxalate of ammonia, a white pulverulent precipitate, proving the presence of lime, which must originally have existed as carbonate or sulphuret. After the oxalate of lime had been removed, phosphate of soda and ammonia produced a white crystalline precipitate, indicating the presence of magnesia.

Quantitative Analysis of Insoluble Salts.

Estimation of Organic Matter.—As has been already mentioned, the specimen of kelp under examination had not been entirely freed from nitrogenous matter. This was discovered while deflagrating a portion of the kelp with nitre, when a

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strong smell of ammonia was given out. At first it was supposed that this might originate from the decomposition of the nitre, but on further investigation it was observed that the kelp when ignited without the nitre, produced the same odour. A quantitative determination of the nitrogen, hydrogen, and carbon, was therefore made.

Estimation of Nitrogen.—The nitrogen was determined in the usual manner by combustion with soda lime, and passing the ammonia through muriatic acid. The muriate of ammonia thus formed was precipitated, by means of the bichloride of platinum, as the yellow ammonia-muriate of the bichloride of platinum, which was thrown on a weighed filter, washed with alcohol, and dried at 212° Fahr.

	Yellow Salt. $NH_3\,HCl.PtCl_2$	Nitrogen.	Nitrogen, per cent.
20 grains gave	2·1	·1317	·6585

Carbon and Hydrogen.—To prepare the carbonaceous matter for analysis, 300 grains of the kelp were carefully washed with distilled water, by which process the soluble salts were removed. The matter which was insoluble in water, was digested in dilute acid, when the insoluble salts were taken up, and organic matter with silica remained unacted on. The carbonaceous matter and silica in 300 grains were equal to 14·46 grains. This residue was then subjected to combustion with oxide of copper. The following are the results:—

		Carbon.	Carbon, per cent.
Amount of carbonic acid obtained	10·12	2·76	·920
		Hydrogen.	Hydrogen, per cent.
... Water obtained	3·47	·433	·144

When the matter insoluble in water was subjected to ignition in a platinum crucible it lost in weight from the dissipation of the organic matter; but along with organic matter a minute quantity of sulphur and carbonic oxide from the decomposition of the carbonate of lime were also driven off, which rendered the results, as far as concerns the organic matter, not strictly accurate.

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	Loss by Ignition.	Loss by Ignition, per cent.
400 grains gave	11·52	2·88
500 ...	12·18	2.437
500 ...	12·15	3·431
	Mean,	2·582

Estimation of Silica and Sand.—A portion of the kelp was weighed out, and the soluble salts washed out with boiling water. When this was accomplished the insoluble salts were dried, and the carbonaceous matter removed by ignition, after which they were dissolved in muriatic acid, which took up the insoluble salts, and left the silica and sand. This residue was then boiled with carbonate of soda, which removed the previously combined silica, and the sand remained.

	Silica & Sand.	Silica.	Sand.	Silica, per cent.	Sand, per cent.
400 grains gave	13·24	7·01	6·23	1·75	1·55
400 ...	13·52	7·12	6·40	1·78	1·60
			Mean,	1·765	1·575

Estimation of Carbonic Acid.—The carbonic acid was determined by introducing the insoluble salts into a flask from which a tube passed into another flask containing barytes water. The carbonic acid was disengaged by the addition of weak muriatic acid to the kelp. The gas passing through the barytes solution yielded a precipitate of carbonate of barytes, which was weighed.

	Carbonate of Barytes.	Carbonic Acid.	Carbonic Acid, per cent.
500 grains gave	102·05	22·91	4·580

Estimation of Sulphur.—The sulphur was estimated by means of the same apparatus as was employed for the estimation of the carbonic acid; but instead of barytes a solution of arsenious acid in caustic soda was used. When the sulphuretted hydrogen was evolved, by means of muriatic acid, it converted the arsenious acid into the tersulphuret of arsenic.

$$AsO_3 + 3\,SH = AsS_3 + 3\,HO.$$

The tersulphuret of arsenic was held in solution by the soda; but when muriatic acid was added the yellow tersulphuret fell. This precipitate was then thrown on a weighed filter and washed with water slightly acidulated with muriatic acid.

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	Tersulphuret of Arsenic.	Sulphur.	Sulphur, per cent.
500 grains gave	5·16	1·932	·386

Estimation of Phosphate of Lime.—The insoluble salts having been dissolved in acid and the silica separated, the phosphate of lime was precipitated by ammonia.

	Phosphate of Lime.	Phosphate of Lime, per cent.
400 grains gave	42·84	10·71
500 ...	52·50	10·50
500 ...	52·30	10·46
	Mean,	10·556

Estimation of Alumina.—To ascertain if the phosphate of lime contained alumina, it was dissolved in acid, and then boiled in an excess of strong caustic soda, which would redissolve any alumina. It was found that it contained a small quantity, which was probably accidentally introduced by the caustic soda, or other reagents.

	Alumina.	Alumina, per cent.
400 grains gave	·748	·185
500 ...	·500	·100
	Mean,	·1425

Determination of Lime.—To the liquid from which the phosphate of lime had been separated oxalate of ammonia was added, when oxalate of lime fell. This precipitate being washed and heated to redness was converted into carbonate.

	Carb. of Lime.	Lime.	Lime, per. cent.
500 grains gave	38·85	21·755	4·351

Estimation of Magnesia.—Having removed the lime by filtration, the magnesia was precipitated by means of phosphate of soda and ammonia, as the ammonia-phosphate of magnesia, which when heated was converted into the diphosphate of magnesia [$2(MgO)PO_5$].

	$2(MgO)PO_5$	Magnesia.	Magnesia, per cent.
500 grains gave	43·70	15·60	3·12

The results of the preceding analyses are comprehended in the following table :—

Nitrogen.	·6585
Hydrogen, ...	·144
Carbon, ...	·920

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Silica,	1·765
Sand,	1·575
Carbonic acid,	4·580
Sulphur,	·386
Phosphate of Lime,	10·556
Alumina,	·1425
Lime	4·351
Magnesia,	3·121
	28·1990

		Lime.	Magnesia.	Carbonic Acid.	Phosphoric Acid.	Silica.	Sulphur.
Carbonate of lime,	2·591	1·451	...	1·14	...	...	...
Phosphate of lime,	10·556	5·376	...	...	5·18	...	...
Sulphuret of calcium,	1·093	·900	...	...	...	...	·386
Silicate of lime,	3·824	2·050	...	...	...	1·765	...
Carbonate of magnesia,	6·554	...	3·121	3·433	...	...	...
Sand,	1·575	...	...	...	...	...	...
Carbon,	·920	...	...	...	...	...	...
Hydrogen,	·144	...	...	...	...	...	...
Nitrogen	·658	...	...	...	...	...	...
Oxygen,	1·152	...	...	...	...	...	...
		...	...	...	...	...	...
	29·067	...	...	...	...	...	...

The oxygen was obtained by calculating the quantity necessary to form water, which being united to the nitrogen, would not be driven off at 212° F.

Analysis of Soluble Salts.

Testing Analysis of Soluble Salts.—Those salts which were soluble in water were, before proceeding to the quantitative analysis, tested qualitatively. The following are the results. On addition of muriatic acid to the solution of the salts, an effervescence took place, with evolution of carbonic acid and sulphuretted hydrogen. Sulphuric acid produced a dark colour in the solution from the liberation of iodine. This, however, disappeared on heating the liquid, fumes of iodine being evolved. After precipitating the sulphurets by sulphate of copper, the addition of a small quantity of sulphuric acid made the liquid slightly turbid from the precipitation of sulphur, proving the presence of a small quantity of hyposulphurous acid. When nitrate of silver was added to a solution of the salts, a black precipitate fell from the formation of sulphuret of silver; but after a portion of the salts had

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been boiled with nitric acid, the precipitate, with nitrate of silver, was white and curdy, indicating the presence of chlorine. Chloride of barium gave a white precipitate, part of which being dissolved with effervescence in nitric acid, indicated the presence of carbonic acid; a white powder remained unacted on by the nitric acid, shewing that the salts contained sulphuric acid. After a portion of the salts had been heated to redness, the addition of bichloride of platinum produced a yellow precipitate, proving the existence of potash salts in the kelp. Oxalate of ammonia caused a slight precipitate of lime; and phosphate of soda and ammonia, after some time, a precipitate of ammonia-phosphate of magnesia.

Quantitative Analysis of Soluble Salts.

Estimation of Sulphuric Acid.—Having separated by filtration a portion of the soluble from the insoluble salts, the sulphuric acid was precipitated in the soluble salts, by the addition of chloride of barium and muriatic acid, to dissolve sulphites and phosphates.

	Sulphate of Barytes.	Sulphuric Acid, per cent.
100 grains gave	14·21	4·89
100 ...	14·34	4·94
	Mean per centage,	4·915

Estimation of Sulphurous Acid.—To a solution of the soluble salts, chloride of barium was added, which precipitated the sulphuric, sulphurous, carbonic, and phosphoric acids, as salts of barytes. This precipitate was thrown on a filter and washed with hot water. The sulphate, sulphite, and carbonate of barytes, which were on the filter, were then treated with nitric acid, which converted the sulphite into sulphate, and dissolved the carbonate. The sulphate of barytes was then washed with water and weighed. The difference between the weight of this precipitate, and that of the sulphate of barytes previously obtained, indicated the amount of sulphate of barytes formed by the action of the nitric acid on the sulphite. From this the sulphurous acid was calculated.

	Sulphate of Barytes.	Sulphuric Acid.	Sulphuric Acid before obtained.	Difference.	Sulphurous Acid.
100 grs. gave	15·75	5·40	4·915	·485	·392

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Estimation of Hyposulphurous Acid.—The soluble salts being separated by means of cold water from the insoluble salts, the sulphurets and carbonates were removed by sulphate of copper. After separating this precipitate by filtration, sulphuric acid was added to the liquid, which decomposed the hyposulphites, sulphurous acid being evolved, and sulphur precipitated. This sulphur was then washed, dried at 212° Fahr., and weighed.

	Sulphur.		Hyposulphurous Acid.	Hyposulphurous Acid, per cent.
400 grains gave	·18	=	·54	·135

Estimation of Sulphur.—The quantity of sulphur in the soluble salts was estimated by deflagrating a portion of the kelp with nitre. By this process all the sulphites, hyposulphites, and sulphurets were converted into sulphates. The sulphuric acid was then precipitated by chloride of barium, as sulphate of barytes, which was weighed, and the sulphuric acid contained in it calculated. It is obvious that the sulphuric acid thus obtained comprehended all the sulphur existing as sulphurets, and sulphur acids in the original kelps. If we subtract from it the sulphuric acid found to exist as such in the kelp, we have remaining the sulphuric acid equivalent to the sulphites, hyposulphites, and sulphurets in the kelp. If, again, we subtract from the last result the calculated quantity of sulphuric acid, equivalent to the sulphurous-hyposulphurous acid, and the sulphuret of the insoluble salts, the remainder will be the sulphuric acid, equivalent to the sulphur of the soluble salts.

	Sulphate of Barytes.	Sulphuric Acid.	Sulphuric Acid, per cent.
200 grains gave	46·55	16·04	8·02
		Original sulphuric acid,	4·920
Sulphuric acid =	Sulphurous acid,	·490	3·10
... =	Hyposulphurous acid,	·191	
...	Sulphur, insol. salts,	·965	
	Total calculated sulphuric acid,		1·446
	Sulphuric acid = Sulphur of sol. salts,		1·654
	Sulphur, soluble salts, per cent.,		·6616

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Estimation of Phosphoric Acid.—To the solution from which the sulphuric acid had been precipitated by chloride of barium and muriatic acid, after separation of the precipitate, ammonia was added, when phosphate of barytes fell.

	Phosphate of Barytes.	Phosphoric Acid.	Phosphoric Acid, per cent.
200 grains gave	2·02	·649	·3245

Estimation of Carbonic Acid.—The carbonic acid in the soluble salts was determined in the same manner as in the insoluble salts, by passing the gas evolved by muriatic acid from the solution of the salts, through caustic barytes dissolved in water. The carbonic acid precipitated the barytes as carbonate, from which the carbonic acid was calculated.

	Carbonate of Barytes.	Carbonic Acid.	Carbonic Acid, per cent.
500 grains gave	48·62	10·9	2·180

Estimation of Chlorine.—A solution of the soluble salts was boiled with nitric acid to convert the sulphurets into sulphates; the chlorine was then precipitated by nitrate of silver.

	Chloride of Silver.	Chlorine.	Chlorine, per cent.
15 grains gave	14·1	3·52	23·40
15 ...	15·5	3·88	25·33
		Mean,	24·365

Estimation of Iodine.—This, which is one of the most valuable constituents of kelp, was determined by the following method, which has yielded results very satisfactory:

A portion of the kelp was exhausted of its iodides, by digestion several times in alcohol. The alcholic solution was then evaporated to dryness, and to convert any sulphuret which might have been taken up by alcohol into sulphate, was deflagrated with chlorate of potash, and kept at a red heat till any iodate that might have been formed by the action of the chlorate of potash, was decomposed. The mass was then dissolved in water, and the iodine precipitated by means of chloride of palladium, as iodide of palladium, which was dried at 212° Fahr., and weighed:

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	Iodide of Palladium.	Iodine.	Iodine, per cent.
1000 grains gave	4·05	2·83	·283
1000 ...	4·35	3·06	·306
500 ...	2·05	1·437	·287
	Mean iodine, per cent.,		·292

Separation of Bromine and Iodine.—To effect the separation of the iodine and bromine, a pound of kelp was treated with alcohol, which dissolved out the bromide and iodide. The alcohol was then driven off through the aqueous solution of the salts. Chlorine was passed in order to decompose the iodide and bromide, the iodine and bromine being set free. This liquor, holding solution of free iodine and bromine, was frequently agitated with ether in a stoppered bottle. The aqueous solution gradually became clear on standing, and ether containing the bromine and iodine floated on the surface. This ethereal solution was then decanted, and saturated with soda, after which it was evaporated to dryness, and heated to redness, to destroy any iodate or bromate. The residual salts were dissolved in water, and the iodine precipitated by chloride of palladium. The iodide of palladium being separated by filtration, the excess of palladium was removed from the filtrate by sulphohydrate of ammonia. It was found in this experiment that sulphohydrate of ammonia answered better than sulphohydric acid for removing the excess of palladium; because when sulphohydric acid is employed, part of the sulphuret of palladium is dissolved by the acid which was previously united to the palladium, which was set free by the sulphohydric acid. Having removed the excess of sulphohydrate of ammonia by boiling, chlorine was again passed through the solution, to decompose bromide. The bromine which was set free was taken up by ether (this had a yellow colour, probably from the presence of a small quantity of bromine). The ethereal solution was then neutralised by soda, evaporated to dryness, and heated to redness. The aqueous solution of the residue gave a white precipitate with nitrate of silver, which consisted principally of chloride of silver. But from the colour of the ether, it was evident that it contained a small quantity of bromine.

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Estimation of Potassium.—To determine accurately the quantity of potash, it was considered advisable to convert any potash that might exist as sulphate into chloride, which was effected in the following manner. From the solution of the salts, the sulphuric acid was precipitated by chloride of barium, and the sulphate of barytes separated by filtration. The excess of barytes was then thrown down by carbonate of ammonia. The liquor, after the carbonate of barytes had been removed, was evaporated to dryness and heated to redness, to expel the ammonia. The residue was dissolved in water, and the potassium precipitated by the addition of the sodium bichloride of platinum, as the potassium bichloride of platinum ($KCl\ PtCl_2$).

	(KCl PtCl2)	Potassium.	Potassium, per cent.
30 grains gave	31·0	5·071	16·

Estimation of Lime.—This was determined by precipitation, as oxalate of lime; the precipitate, when heated, was converted into carbonate of lime.

	Carbonate of Lime.	Lime.	Lime, per cent.
400 grains gave	1·49	·805	·201
500 ...	2·33	1·300	·260
	Mean lime, per cent.,		·230

Estimation of Magnesia.—The magnesia was precipitated from the solution of the salts by phosphate of soda and ammonia, as ammonia-phosphate of magnesia, which was converted by heat into the diphosphate of magnesia.

	Diphosphate of Magnesia.	Magnesia.	Magnesia, per cent.
500 grains gave	3·61	1·312	·264
400 ...	3·00	1·071	·267
	Mean magnesia, per cent.,		·277

Results of Analyses of Soluble Salts.

Sulphuric acid, ...	4·915
Sulphurous acid, ...	·392
Hyposulphurous acid, ...	·135
Sulphur, ...	·6616
Phosphoric acid, ...	·3245

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Carbonic acid,	2·180
Chlorine,	24·365
Iodine,	·292
Bromine,	trace.
Potassium,	16·000
Lime,	·230
Magnesia,	·277
	49·754

The deficiency here is the soda, as may be seen by the two following Tables, in which the acids and bases are united according to their affinities; and the result of the examination of a hundred parts of kelp is given, comprehending both soluble and insoluble matter:—

	Sulphuric Acid.	Sulphurous Acid.	Hyposulphurous Acid.	Sulphur.	Chlorine.	Potassium.	Sodium.	Lime.	Magnesia.
Sulphate of potash,	2·058	...	...	...	...	2·058	...	...	...
Sulphate of soda,	2·000	...	...	...	...	...	1·06	...	...
Sulphate of magnesia,	0·693	...	...	...	...	...	...	...	·231
Sulphate of lime,	0·164	...	...	...	...	...	...	·115	...
Sulphite of soda,	...	·392	...	...	...	...	·261	...	...
Hyposulphite of soda,	...	...	·132	...	...	...	·058	...	...
Sulphuret of sodium,	...	...	...	·6616	...	...	·9894	...	...
	Phosphoric Acid.								
Phosphate of soda,	·3245	...	...	...	...	...	·162	...	...
	Carbonic Acid.								
Carbonate of soda,	2·180	...	...	...	...	...	1·846	...	...
Chloride of potassium,	...	...	...	...	12·584	13·943	...	...	...
Chloride of sodium,	...	...	...	...	11·570	...	7·764	...	...
Chloride of calcium,	...	...	...	...	·147	...	...	·116	...
	Iodine.								
Iodide of magnesium,	·292	...	...	...	..	...	...	...	·040
	Bromine.								
Bromide of magnesium,	trace.	...	...	...	...	...	...	...	...

Table of Per-Centage Composition of Orkney Kelp.

Insoluble Salts.

Carbonate of lime,	2·591
Phosphate of lime,	10·556
Oxysulphuret of calcium,	1·093
Silicate of lime,	3·824
Carbonate of magnesia,	6·554
Sand,	1·575

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Alumina,	·142	
Carbon,	·920	
Hydrogen,	·144	
Nitrogen,	1·152	
Oxygen,	·658	
		29·209
Soluble Salts.		
Sulphate of potash,	4·527	
Sulphate of soda,	3·600	
Sulphate of lime,	·279	
Sulphate of magnesia,	·924	
Sulphate of soda,	·784	
Hyposulphite of soda,	·220	
Sulphuret of sodium,	1·651	
Phosphate of soda,	·540	
Carbonate of soda,	5·306	
Chloride of potassium,	26·491	
Chloride of sodium,	19·334	
Chloride of calcium,	·229	
Iodide of magnesium,	·316	
Bromide of magnesium,	trace.	
Water,	6·800	
		71·000
		100·209

On the Colours of a Jet of Steam.

Professor J. D. Forbes, some years ago, observed that a jet of steam absorbed the more refrangible portion of white light.* It happened, during some experiments, that a blue jet of steam caught my attention, and further experiments soon assured me that it was easy to obtain a jet of almost any colour.

A blowpipe jet was screwed on a T-piece, and the opposite opening of the T-piece was supplied with a stopcock, while the third opening of the T-piece communicated, by means of a tube, with the cock of the boiler. The blowpipe jet had an orifice about $\frac{3}{100}$ths of an inch diameter, and its axis was elevated about 28° above the horizon. The stopcock on the T-piece was furnished with a little contrivance for preventing the steam that it discharged from interfering with the appearance of the steam discharged by the blowpipe jet; the use of this stopcock was to blow off the water which condensed in the steam passages. A pressure was maintained in the boiler of about 40 lbs. on the inch.

On fully opening the cock of the boiler a jet of steam was obtained

* Philosophical Magazine, S. 3, vol. xiv., p. 121.

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which appeared blue in nearly every position in which it could be viewed. Looking end-on from below, the steam jet caused that part of the heavens obscured by it to appear feebly orange coloured. The day was bright, but the sky at this quarter was overcast. On looking through the jet of steam from below upwards, but in a direction inclined about 11° to the axis of the jet, in which position a portion only of the steam-cloud could be viewed by the direct light of the clouds, the remaining portion being sheltered by the side of the window, one part of the jet appeared orange red, namely, that part which transmitted the direct light of the clouds, while the other portion was blue. The blueness of the jet increased with the above-mentioned angle until the angle was perhaps 30°, after which the blueness somewhat diminished, but was far from being extinguished at 90°.

By partly closing the cock of the boiler, and so discharging steam from the jet of, perhaps, not a higher pressure than 10 lb. on the inch, I could obtain a jet of steam, which, looking end from below, was blue. It was rather difficult to obtain this blue jet, and when obtained, it kept alternating with violet. On now viewing this blue jet under an angle as before (192) of about 20°, it appeared reddish-orange in colour; this colour was not visible at almost any angle, like the reflected blue (192).

Looking end-on, and adjusting the pressure, I have occasionally seen for a moment at a time a bright green jet; also, and commonly a blue-purple. In the reflected tints I am not sure that I have seen any thing more than orange-red, violet, and blue. The transmitted colour appeared in my experiments more intense than the reflected tints. This, perhaps, has its explanation in the fact, that when looking end-on, the eye receives light which has shone through a columnar arrangement, whose length is much greater than its diameter, while the reflected lights would only be seen by looking on the convex surface of the columnar stream of particles.

Prof. Forbes, after discovering the red colour of a jet of steam by transmitted light, connected the red colour of the clouds with this fact; and the truth of this connection is beyond dispute. So far, however, as I have been able to go the colours of the steam jet are manifestly only instances of ordinary interference, greatly resembling that produced by thin transparent plates; the transmitted ray being always complementary to the reflected. Thus in 192 the transmitted light is red, as in Prof. Forbes's experiments, but the reflected light is blue. It is therefore to be inferred that all the colours of the clouds originate in interference caused by minute drops of water, the size of which determines their colour; while the blue jet (192) is, I think, strictly analogous to the blue sky.*

* Phil. Mag., Aug. 1852.

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Report upon the Alleged Adulteration of Pale Ales by Strychnine. By Professors GRAHAM and HOFFMANN.

Having undertaken, at the request of Mr Allsopp, an inquiry into the purity of bitter beer, with particular reference to its alleged adulteration by strychnine, we now submit the results which we have obtained upon the subject.

Strychnine or strychnia, the alleged substitute for the hop, is a fine crystallisable substance, extracted from *Nux vomica*, and belonging to the class of vegetable principles termed alkaloids, of which quinine from Peruvian bark, and morphine from opium, are the most familiar examples. These substances, although susceptible of the most valuable medical application in small doses, are, generally speaking, remarkable for their energy as poisons, and for the intense bitterness of their taste; two properties which are developed in strychnine in the highest degree. Half a grain of the latter substance would poison, and the bitterness of the same minute quantity is perceptible in every drop of six or eight gallons of water in which it is dissolved.

It may be stated at once, that the quantity of strychnine, which we find necessary to impart to beer the degree of bitterness possessed by pale ales, is for a gallon of beer one grain of strychnine, or double the fatal dose. The price of strychnine is about sixteen shillings the ounce, which does not amount to so much as one penny per grain. Estimating the annual production of pale ale in Burton at 200,000 barrels, the strychnine required as a bitter would, however, amount to 16,448 ounces, and cost £13,158; while nobody believes that so much as 1000 ounces of strychnine are manufactured over the whole world. The bitterness obtained by means of strychnine is equal in degree to that of the hop, but very different in kind, and easily distinguished when the two bitters are compared. The bitter of the hop is immediate in its action upon the palate, is accompanied by a fragrant aroma, and soon passes off; whilst that of strychnine is not so instantaneous; but when the impression is once made it is more lasting, and becomes, from its persistence, like that of a me-

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tallic salt. The bitter of strychnine is, indeed, easily distinguishable from that of the hop, when deliberately tasted.

Still it would be highly desirable to be able to identify strychnine in beer, by the actual extraction of the substance, and the application to it of a chemical test of absolute certainty. Fortunately those poisons which have the most violent action upon the animal economy, possess often also the best marked reactions, or their physiological and chemical properties are equally salient. Thus, arsenic and hydrocyanic acid are the most easily detected of chemical substances; and strychnine proves to be not far behind them in this respect.

A quantity of strychnine, not exceeding $\frac{1}{100}$th of a grain, is tested and recognised to be strychnine in the following manner. The powder is moistened with a single drop of undiluted sulphuric acid, and a small fragment of chromate of potash placed in the liquid. A beautiful and most intense violet tint immediately appears at the points of contact, and is speedily diffused over the whole liquid. Although most intense, the colour disappears entirely again in a few minutes. The admixture of the smallest quantity of organic matter, however, interferes with the success of the process. In order to apply the test, in operating upon a complex liquid like beer, the strychnine must first be extracted from the liquid and obtained in a pure or nearly pure condition. This difficulty, which appears at first considerable, may be readily surmounted, and the strychnine, if it really exist in beer, be separated, and its nature established in the most certain manner.

For this purpose, two ounces of ivory black, or animal charcoal were shaken in half a gallon of beer, to which half a grain of strychnine had been purposely added. After standing over night, the liquid was found to be nearly deprived of all bitterness; the strychnine being absorbed by the charcoal. The liquid was now passed through a paper filter, upon which the charcoal containing the strychnine was collected and drained.

The next step was to separate the strychnine from the charcoal. This was readily effected by boiling the mixture for half an hour in eight ounces of ordinary spirits of wine,

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avoiding loss of alcohol by evaporation. The spirits which now contained the strychnine were next filtered, and afterwards submitted to distillation. A watery fluid remained behind, holding the strychnine in solution, but not sufficiently pure for the test. The final purification was accomplished by adding a few drops of potash to the watery fluid, and then shaking it with an ounce of ether. A portion of the ethereal solution evaporated upon a watch glass, left a whitish solid mass of intense bitterness, and this was recognised to be strychnine, by giving the violet tint previously described upon the application to it of sulphuric acid and chromate of potash.

Having satisfied ourselves by repeated experiments with samples of beer, to which strychnine had been previously added, of the never-failing efficiency of the above method of extraction, we now proceeded to the actual examination of the commercial article. With this object a series of samples were taken indiscriminately from the stores of a considerable number of the London bottlers, whosupply the public with Allsopp's pale ale.

It may be stated that with the exception of five varieties, the casks from which these samples were taken had all been received in London before the 20th of March, *i. e.*, the period when the possible use of strychnine in the manufacture of bitter beer was first brought before the English public.

Not one of these varieties of beer, when tested with the greatest scrupulousness, gave the slightest evidence of the presence of strychnine.

The charge of adulteration of beer by strychnine has been proposed in a manner so vague, that it is difficult to fix it, and try its validity. The existence of the adulteration is not alleged in any particular sample of beer, nor the practice ascribed to any individual brewer or dealer. An English journalist adopts the charge, upon the report that such an opinion is entertained and expressed by a French chemist of distinction, M. Payen, in his public lectures at Paris. From this gentleman we have since obtained explanations which define more closely the kind of charge which was actually made by him. The late M. Pelletier, the well-known manu-

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facturer of organic products in France, had received at one time an order for an extraordinary quantity of strychnine, of which the destination was at first unknown to him; but which he afterwards learned had been entirely exported to England, and used, as he informed M. Payen, to complete the bitter of certain kinds of beer.

We have reason to know, although it is not stated by M. Payen, that these remarks of Pelletier refer to a period ten or twelve years past; and further, although not informed of the amount of the order, we have got good authority to state that fifty or a hundred ounces would have been considered a large order for strychnine at that time. The calculation already given shews how utterly insignificant such a supply of strychnine would be for its imagined application in the pale ale breweries. It is likewise known that the manufacture of strychnine has not been on the increase in France of late years.

M. Payen excuses his statements on the ground that similar suspicions are conveyed in a French work "On Adulterations and Falsifications," by Chevallier, published nearly a year ago; but which have not hitherto received any formal contradiction in England. Notwithstanding the latter circumstance, our distinguished correspondent concludes by expressing his regret that he ever said "that the fraud appeared to have been practised," although he had added the remark at the time, "that this falsification had no doubt ceased."

It thus appears, that the charge which has been put into the mouth of M. Payen, was never made at all by that gentleman, so far as it applies to the present practice of English brewers, and with reference to anterior times, that the charge reposes simply and exclusivly upon the privately expressed opinion of a deceased chemist, the grounds of which are entirely unknown to the world, and must ever remain so.

In conclusion, it is scarcely necessary to refer to the sifting nature of the chemical examination which the beer of Messrs Allsopp's manufacture for months past have been subjected to, and which establish their incontestible purity. Indeed, no one who has witnessed, as we have done, the open manner and gigantic scale in which the operations are conducted in

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their establishment, would entertain the idea for a moment that any practice involving concealment was possible. But even in the absence of all such scrutiny, the idea of strychnine being mixed with beer anywhere, or in any circumstances, involves an amount of improbability which might well dispel all suspicion on the subject.

There is an Act of Henry VII., which prohibits the adulteration of ale by brimstone or *hops*. The place of the hop was then supplied by sage, horehound, chamomile, and other indigenous bitter plants. Since that period, the character of the national beverage must have undergone a silent revolution, for all varieties of beer, both pale and brown, now owe their distinctive properties to the hops which are boiled in the malt infusion, and fermented along with it, as completely as wine owes its peculiar character to the grape ; substitute any other bitter for the hop, and the fermented wort would no longer be recognised as beer.

Were mere bitters all that is required, it would be easy to prove that the extract of quassia would supply a bitter which is perfectly harmless and agreeable, and infinitely less expensive than strychnine.

But the process of brewing pale ale is one in which nothing but water, the best malt, and hops of the first quality are used, and is an operation of the greatest delicacy and care, which would be entirely ruined by any tampering with the materials employed. Strychnine could not fail to be rejected, from the ungrateful metallic character of its bitterness, independent of all objections of a more serious kind. This peculiarity of taste is also calculated to betray its presence. Small, too, as the proportion of strychnine may be, which is necessary to impart the degree of bitterness of pale ale, the quantity rises, as has been seen, to a poisonous dose in half a gallon of the fluid ; and as this poison is one of those which are known to accumulate in the system, its poisonous action would inevitably follow, in occasional cases, upon the consumption of much smaller portions of beer when continued for many days without intermission. The violent tetanic symptoms of poisoning by strychnine are also such as could scarcely fail to excite suspicion and alarm. Add to these

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disadvantages, the certainty of the means of detecting strychnine in beer by the chemical tests described above, which any medical man or practical chemist can apply, and the chance of the use of so dangerous a substance for any purpose of adulteration, becomes in the last degree improbable.*

[The following letter on the alleged adulteration of bitter beer, from Professor Liebig to Messrs Allsopp, inserted in the *Times*, will interest our readers.]

"The unguarded remark of a French chemist, that the strychnine imported in England, is employed in part as a substitute for hops in the manufacture of beer, has lately spread alarm among the lovers of pale ale. Having been appealed to by you to express my opinion on this subject, which appears to me to be, in a dietetic point of view, one of considerable public interest, I now offer the following brief statement.

"About a quarter of a century ago, a brewer in Westphalia fell into the practice of adulterating his beer with *Nux vomica*, from which it is well known that strychnine is obtained. The peculiar morbid symptoms, however, which resulted from the consumption of this adulterated beer, speedily led to the detection of the fraud. The effects produced by *Nux vomica* and strychnine are so characteristic, that every medical man will readily detect their origin. The French novelist, Alexandre Dumas, has described them, though with more imagination than truth, in his romance of 'Monte Christo.' It is possible that the Westphalian case, which, from being made the subject of a criminal trial, obtained great notoriety, has given rise to the assumption, that in England the strychnine imported is used for the purpose of mixing with beer. But nobody at all acquainted with the great breweries of that country could seriously entertain the suspicion of an adulteration of beer with strychnine or any deleterious substance. It is practically impossible that any operation of a doubtful character could be carried out in these extensive

* Quarterly Journal of the Chemical Society, vol. v., No. 18, p. 173.

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establishments, on account of the large number of workmen employed in them. Any attempt on the part of the brewer to impart to his beer in an illicit manner qualities which are not to be obtained from malt or hops, would necessarily lead to his ruin; as he would be obliged to communicate his secret to too many persons, and to employ too many accomplices. The draymen themselves, as good connoissieurs in beer, would protest against any manipulation of a suspicious character. The case has even occurred of an eminent brewer not venturing to make use of a method suggested to him, for the purpose of clearing his beer more effectually, because the addition of a new material to the wort might have induced a suspicion in the minds of his workmen that it was an illicit proceeding, and this would have endangered the good reputation which his beer enjoyed. He stated to me at the same time that no improvement could be introduced into a brewery, the object of which was not perfectly evident to every body.

"During a sojourn of several days at Burton-on-Trent, I had an opportunity of becoming intimately acquainted with the method pursued in the manufacture of pale ale. I convinced myself that the qualities of this excellent beverage depended mainly upon the care used in the selection of the best kinds of malt and hops, and upon the ingenuity exhibited in conducting the processes of mashing and fermenting. Our continental brewers have much to learn in these points to come up to the English brewers. I have no hesitation in saying, that England possesses the greatest adepts in malting. I know positively, that the chief brewers of Munich, who undoubtedly produce the best beer in Germany, have gone through an apprenticeship in Burton. This may account for the predilection entertained by the general public, as well as by medical men, for these varieties of beer; for the instinct of humanity and experience appear to be as good guides in the choice of things that contribute to health and enjoyment as the profoundest philosophy.

" Professors Graham and Hoffmann, in the excellent report already addressed to you upon the alleged adulteration of the pale ale by strychnine, have indicated a very simple process

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for detecting the most minute quantity of strychnine contained in beer. I have satisfied myself of the great convenience and accuracy of their method, and have further assured myself, by an analysis of several specimens of pale ale obtained from London houses, supplied by your establishment, of the utter groundlessness of the imputation, that this beer was poisoned with strychnine. I am positive, and am supported in my views by the concordant analyses of all chemists who have occupied themselves with the examination of beer, that the poisoning of pale ale with strychnine has never occurred. I believe I may safely add, that it never will take place; for although an ignorant brewer might be induced from interested motives to add *Nux vomica* to his beer, the word strychnine so forcibly suggests one of the most virulent poisons, that whoever has heard anything about strychnine at all, is sure to be aware of this. By adulterating his beer with strychnine, the brewer would be knowingly committing a crime which, in the present state of science, must be followed by immediate detection and punishment.

" Mr E. Merck of Darmstadt, one of the most extensive strychnine manufacturers in Europe, informs me that this substance is peculiarly adapted to destroy vermin of all kinds. In many parts of Germany it is the popular poison for rats and mice. This fact fully accounts for the large amount of the drug that has lately been introduced into commerce.

" The specimens of your pale ale sent to me have afforded another opportunity of confirming its valuable qualities. I am myself an admirer of this beverage, and my own experience enables me to recommend it, in accordance with the opinion of the most eminent English physicians, as a very agreeable and efficient tonic, and as a general beverage both for the invalid and the robust.

" JUSTUS LIEBIG."

" Giessen, May 6, 1852."

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The New Metal Donarium is Thorina.

A few months ago, M. Bergemann discovered an oxide in a mineral from Langesundfiord, near Brevig, in Norway, which he considered to be new. He gave the name Donarium to the metal, and Dr Krantz that of Orangite to the mineral.

Damour has since examined a specimen of orangite. Its specific gravity was 5·19, Bergemann found 5·39. On comparing his analysis with that of Bergemann, and also the properties of the supposed new oxide, M. Damour concludes, that the oxide of donarium is nothing less than impure thorina; Bergemann's analysis does not enumerate oxide of lead and oxide of uranium among the constituents. M. Berlin of Lund, has also found that the oxide of donarium is thorina mixed with minute traces of oxide of uranium, oxide of iron, vanadic acid, tin, and perhaps a little molybdic acid. The following are the analyses :—

Damour.		Berlin.	
Silica,	17·52	Silica,	17·78
Thorina,	71·65	Thorina,	73.29
Lime,	1·59	Lime,	0·92
Oxide of Lead,	0·88	Oxide of Uranium, Peroxide of Iron, Tin, Vanadium,	0·96
Oxide of Uranium,	1·13		
Oxide of Manganese,	0·28		
Peroxide of Iron,	0·31		
Magnesia,	trace		
Alumina,	0·17		
Potash,	0·14		
Soda,	0·33		
Water with trace of Carb. Acid,	6·14	Water,	7·12
	100·14		100·00

Damour deduces from his analysis the formula $3\,Th\,O + Si\,O^3 + 2\,H\,O$. Berzelius assumed that thorite consisted of several silicates, but principally of a silicate thorina, of the formula $3\,Th\,O + Si\,O^3 + 2\,H\,O$. Damour is of opinion that Berzelius's analyses do not lead to any definite proportion; but they prove that orangite and thorite are identical, and that the metal donarium must be struck from the list of simple bodies. Berlin also calculates from his analysis the formula

$$3\,Th\,O + Si\,O^3 + 2\,H\,O$$

and is likewise of opinion that orangite is only a purer thorite. He also draws attention to a peculiar property of thorina. It is stated that calcined thorina is insoluble in acids. This is correct as far as regards the earth obtained by calcining the hydrate, but not for that obtained by igniting the oxalate, which dissolves slowly in hydrochloric acid. (*Central Blatt, June* 23, 1852; *and translated in Philosophical Magazine*, vol. iv., No 23, 4th Series, p. 156.)

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Chemico-Geological Researches on the Sulphurets which are Decomposable by Water. By E. FREMY.

The object of this paper, says the Comptes Rendus for July 5, 1852, is to make known the production and principal properties of a class of sulphurets, hitherto little examined, and the study of which is alike interesting to chemists and geologists, from the light which it throws on the formation of mineral waters.

When we consider, says Mr Fremy, the action of water on the sulphurets, we find that these compounds may be divided into three classes: the first comprises the sulphurets of the alkalies and of the alkaline earths which dissolve in water; the second is formed of the insoluble sulphurets; the third consists of the sulphurets of boron, silicon, magnesium, and aluminum, which are decomposed by water; these latter are scarcely known, owing to their preparation having hitherto been acompanied with great difficulties. In order to a thorough investigation of all the questions which are connected with the decomposition of the sulphurets by water, I first sought for a method by which they might be easily prepared. This method I will now describe.

It is well known that sulphur exerts no action upon silica, boracic acid, magnesia, and alumina. I imagined it might be possible to replace the oxygen in these substances by sulphur, by the intervention of a second affinity, as that of carbon for oxygen. Such decompositions, produced by two affinities, are not rare in chemistry; and in some yet unpublished experiments on the fluorides, I had observed that the sulphuret of carbon completely decomposed the fluoride of calcium mixed with silica, producing sulphuret of calcium. I was therefore led to presume that the sulphuret of carbon, acting by its two elements upon the preceding oxides, would remove the oxygen, by means of the carbon which it contains, and would, at the same time, form sulphurets; this supposition I found confirmed by experiment. In fact, I have obtained the sulphurets of boron, silicon, magnesium, and aluminum, by submitting boracic acid, silica, magnesia, and alumina, to the

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action of sulphuret of carbon at a high temperature. To facilitate the reaction, and remove the sulphuret from the decomposing action of the alkalies contained in the porcelain tubes, it is sometimes useful to mix the oxides to be reduced with charcoal, and to form them into little balls similar to those which are used in the preparation of the chloride of silicon.

I have ascertained by analysis that these sulphurets correspond to the oxides from which they have been derived.

I will now say a few words on the sulphurets obtained by the above method. The sulphuret of silicon had been obtained in small quantity by Berzelius in the reaction of sulphur upon silicon, and by M. Pierre in the decomposition of chloride of silicon by hydrosulphuric acid. I have obtained this substance with the greatest ease, by passing the vapour of sulphuret of carbon over pellets of charcoal and gelatinous silica, placed in a porcelain tube heated to bright red. The sulphuret of silicon condenses in the tube in beautiful white silky needles, which are not very volatile, but are readily carried along by the vapour.

To shew the interest which attaches to the examination of this substance, it will suffice to mention here two of its reactions. When sulphuret of silicon is heated in a current of moist air, it is decomposed, and furnishes silky crystals of anhydrous silica; it is evident that we may explain, by means of this experiment, the natural production of certain filamentous crystals of silica. The sulphuret of silicon in the presence of water is decomposed with a brisk evolution of hydrosulphuric acid into silica, which remains entirely dissolved in the water, and is not deposited until the liquid is evaporated. It is impossible not to connect this curious property with those natural conditions under which certain mineral waters and siliceous incrustations are formed.

As the sulphuret of silicon is probably produced in all those cases where silica is submitted to the double action of a binary compound which cedes sulphur to it, and at the same time appropriates its oxygen, this sulphuret is probably not so rare as has been hitherto thought; and, by admitting its presence in those rocks in which sulphurous springs occur,

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we might explain the simultaneous existence of silica and sulphuretted hydrogen in the principal sulphurous waters. This hypothesis is in some measure confirmed by the interesting observations of M. Descloizeaux, which shew that the siliceous springs of the Geysers of Iceland contain a large quantity of sulphuretted hydrogen.

I content myself with submitting these considerations to geologists, merely observing that in explaining the formation of sulphurous and siliceous waters by the decomposition of the sulphuret of silicon, I am only extending the ingenious theory proposed by M. Dumas, to explain the formation of boracic acid.

The sulphurets of boron and aluminum were prepared like the sulphuret of silicon, and are likewise decomposed by water.

The sulphuret of magnesium I obtained by passing sulphuret of carbon over pure magnesia; in this case the presence of charcoal does not appear to be of any use. This sulphuret crystallises, and is soluble in cold water. When its solution is kept at the ordinary temperature, there is but a feeble disengagement of sulphuretted hydrogen; but when heated to ebullition, a lively effervescence of sulphuretted hydrogen takes place, and there is an immediate deposition of magnesia.

Analyses of Indian Ores of Manganese, and of some Scottish Zeolites. By Dr A. J. Scott, H.E.I.C.S. Communicated by the Author.

Among a number of minerals which were kindly sent to me by Dr Alexander Hunter of Madras from different localities in India, I have examined several during the course of last winter in the laboratory of Dr Anderson of Edinburgh, and under his immediate superintendence. Those which present most interest in a mineralogical point of view are two manganese ores found at Vizianagram and Bimlapatam, in the northern Circars. It would seem that the former occurs in very large quantities. A description of it by Dr

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Hunter has appeared in several of the Madras journals, but as no analysis of it has as yet been published, and as it belongs to a class of manganese minerals of rather rare occurrence, a short notice of it may not be devoid of interest.

It occurs in large irregular masses, some of them described to be of several tons weight. I am not acquainted with the geological formation in which this mineral is met with, never having visited that part of India, but, generally speaking, the prevailing rock in the Indian Peninsula, especially of the Carnatic, in which many minerals containing iron or manganese occur, is that which is commonly known by the name of "Laterite," a rock peculiar to that country, and of which an excellent detailed description has been already published by the late Captain Newbold of the Madras army.

The mineral under consideration presents a highly metallic lustre of a bluish-black colour, interspersed here and there with dull greyish spots, which latter possess the external character of Psilomelan. It breaks with difficulty, and when split with a chisel presents an imperfect rhombohedral cleavage. Its specific gravity is 4·50.

Its powder is of a dark brownish-black colour, which dissolves readily in hydrochloric acid, with the evolution of chlorine gas, and on evaporation forms a gelatinous mass of a deep yellow colour. Its analysis was performed by dissolving it in hydrochloric acid, evaporating to dryness, and heating strongly, in order to render the silicic acid insoluble, and effect its separation. The iron was separated from the manganese by succinate of ammonia; the latter metal being then precipitated by hydrosulphuret of ammonia, was afterwards redissolved and thrown down by carbonate of soda, and ultimately reduced to red oxide by subjecting it to a strong heat, in which state its weight was ascertained. The other ingredients were determined in the usual manner. The quantitative constitution of the mineral was found to be as follows:—Silicic acid, 8·300; peroxide of iron, 12·910; magnesia, 2·339; water, 0·539; red oxide of manganese, 73·786; oxygen, 1·864; total, 99·735. The quantity of metallic manganese in the above analysis amounts to 53·428

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per cent., and the total quantity of oxygen combined therewith to 22·219 per cent., which corresponds very closely to the constitution of sesquioxide, or of a mixture of nearly equal quantities of protoxide and peroxide, as shewn by the annexed calculation.

Mn	53·428	= Mn O	39·050	i.e. Mn	30·2686	+ O	8·7814
O	22·219	= Mn O_2	36·597	i.e. Mn	23·1594	+ O	13·4376
	75·647		75·647		53·428		22·219

On comparing the composition of this mineral with those containing manganese, of which analyses have been already published, it is found to agree most nearly with a manganese ore called Marcellin from St Marcel in Piedmont, and which has been investigated by Damour. This observer considers the mineral he analysed to be a mixture of Braunite and silicate of the protoxide of manganese; but Rammelsberg very properly remarks, that if it possess a distinct crystalline form, which it appears to do, it cannot be a mixture, and suggests, as more probable, that the crystals may be Braunite, and that the analysis has been made with a specimen containing impurities.

The manganese ore from Bimlapatam, a station not far distant from Vizianagram, is very similar, if not identical to the foregoing in its external and chemical characters. It differs from it, however, to some slight degree, and was found to contain lime, which the other does not. Its quantitative analysis gave the following results:—Silicic acid, 9·09; peroxide of iron, 11·72; lime, 1·244; magnesia, 0·668; water, 0·432; red oxide of manganese, 76·177; oxygen, 0·655; total, 99·986. The quantity of metallic manganese indicated in the above quantity of red oxide would be 54·929, that of the oxygen of the same, together with the free oxygen added, to 22·558, whereas, in order to constitute sesquioxide, 23·904 of oxygen would be required for the same quantity of metallic manganese. It would thus appear that the metal in this case must be in a lower state of oxidation than in the Vizianagram specimen.

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Analysis of Scottish Zeolites.

Pectolite.—The first of the series is a mineral which occurs in the Island of Skye at Storr, which, in its external characters, bears a considerable resemblance to dysclasite, for which I at first took it. It is met with in compact fibrous masses, composed of radiated needles, of extremely minute size and silky lustre. It is exceedingly tough, and breaks with difficulty. Its specific gravity is 2·784. Before the blowpipe it fuses, without intumescence, into a bead, and also gives slight indications of the presence of alumina and manganese.

On comparing it with an undoubted specimen of dysclasite, however, it evidently presents a much higher lustre than that mineral possesses, although in other respects there is but little or no observable difference in the external appearance of the two minerals. It is partially soluble in hydrochloric acid, with the aid of heat, viscid flakes of silicic acid being separated; and in this particular it agrees with Von Kobell's account of a specimen of pectolite from Monte Baldo, of which he has published an analysis. On a qualitative examination, it was found to contain silica, lime, soda, alumina, and water; and its quantitative analysis was very simple, the results being as follows:—Silicic acid, 52·007; alumina, 1·820; lime, 32·854; magnesia, 0·396; soda, 7·670; water, 5·058=99·805. If we exclude as unessential the small quantities of alumina and magnesia found in this analysis, the oxygen of the silicic acid, lime, soda, and water, is in the ratio of 12 : 4 : 1 : 2, and would indicate that the mineral is a compound of a neutral silicate of lime, with a basic silicate of lime and water, giving for its formula,

$$Na\,O\,Si\,O_3 + (4\,Ca\,O\,3\,Si\,O_3) + 2\,HO.$$

The calculated results of this combination are,

Silicic acid,	4 atoms,	181·2	–	52·6
Lime,	4 ...	112·0	–	32·4
Soda,	1 ...	31·3	–	9·1
Water,	2 ...	18·	–	5·9
		342·5		100·0

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This calculation presents a very close agreement with the experimental results in all the constituents, with the exception of the soda, which differs to some extent, but still so little as to render it obvious that the above must be its formula.

I have called this mineral Pectolite, because its chemical composition and its external character, so far as they can be determined by the description given in books, agree very closely with those of the mineral described by Von Kobell under that name; but not having seen a specimen of the true mineral from Monte Baldo, I cannot pronounce upon it with absolute certainty. Analyses of the true pectolite, and of another mineral occurring at Royal Island, in Lake Superior, in North America, have been already published, and their results are as follow :—

	Monte Baldo. Von Kobell.	Royal Island. A.	B.
Silicic acid, .	51·30	53·45	55·66
Lime, . . .	33·77	31·21	32·86
Alumina, . .	0·90	4·94	1·45
Soda, . . .	8·26	7·37	7·31
Potash, . .	1·57	...	...
Water, . .	3·89	2·72	2·72
	99·69	99·69	100·00

These analyses are somewhat conflicting; but the first, by Von Kobell, approximates very nearly to my analysis of the Skye mineral. From the former Berzelius has deduced the somewhat complicated formula :—

$$3\,(Na\,O\,Si\,O_3) + 4\,(3\,Ca\,O\,2\,Si\,O_3) + 3\,HO$$

The calculation of which gives—

Silicic acid,	11 atoms	=	508·442	=	52·603
Lime,	12 ...	=	337·584	=	34·927
Soda,	3 ...	=	93·534	=	9·667
Water,	3 ...	=	27·	=	2·793
			966·560		

which certainly does not agree by any means so well

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with the experimental results as the formula I have given above. As far as the Royal Island mineral is concerned, however, the concordance is anything but satisfactory, the quantity of silicic acid being much in excess of that contained in either the Monte Baldo or Skye mineral. It appears to me that there can be little doubt that the mineral I analysed is actually pectolite; at the same time I should not wish to express too decided an opinion on the subject, as a late experimenter (Frankenheim) has stated that pectolite is an anhydrous mineral, and that the proportion of water varies in different specimens, and consequently, in his opinion, hygrometric. However this may be, the Skye mineral is certainly an hydrated one, according to my analysis, and several determinations always shewed the same amount of water.

The discovery of this mineral in Skye forms an interesting addition to the mineral species of Scotland, where it has not before been observed.*

Scolezite.—The next is a mineral which is found in the Island of Mull. It occurs in long radiated needles, of great beauty and high lustre, contained in greenstone or trap-rock, with crystals of epidote disseminated through it.

It presents the characteristic properties of a zeolite, curling up before the blowpipe into a vermicular shape. It is completely soluble in hydrochloric acid, and is partially so in a solution of oxalic acid, oxalate of lime being precipitated. Its external and chemical characters correspond with those of scolezite, and its quantitative analysis generally agreeing with those of that mineral, as obtained by Fuchs and Gehlen:—Silicic acid, 46·214; alumina, 27·00; lime, 13·450; water, 13·780 = 100·444.

There is a slight excess of alumina, but, with this exception, the analysis accords with the formula of scolezite—

$$\mathrm{Ca\,O\,Si\,O_3 + Al_2\,O_3,\,Si\,O_3 + 3\,HO},$$

its calculated constitution being as follows:—

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* This species occurs, although rarely, in cavities of trap-rocks, on the banks of the Clyde.—*Edit., N. P. Journal.*

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Silicic acid,	2	atoms	=	92·444	= 46·47
Alumina,	1	...	=	51·344	= 25·81
Lime,	1	...	=	28·132	= 14·14
Water,	3	...	=	27·	= 13·58
				198·920	100·

Several analyses of this mineral have been already published, and are contained in the mineralogical works of Ramelsberg, Von Kobell, and others, on comparing which with the results I obtained from the Mull specimen, they will be found to agree generally with the whole of them, but most closely with those of Gülich and Gibbs of the Iceland variety, of Domeyko of the Cachapual specimen, and of Fuchs and Gehlen of the mineral from Staffa and Faröe.

Natrolite.—This mineral was found during the formation of a railway tunnel near Bishoptown, Renfrewshire. The specimen which I analysed was composed of beautiful needle-shaped crystals, about two inches in length, of a pure white colour and satiny lustre, interlaced into a felty mass. It occurs in juxtaposition with another mineral, which was at first supposed to be identical with it, consisting of long radiated needles, with crystals of calcareous spar disseminated here and there through them, and having a great resemblance to the scolezite which I had previously analysed. A quantitative analysis of this mineral has since proved it to be Mesolite, or lime and soda mesotype. The three minerals in question indeed seem to be isomorphous, and bear such a resemblance to one another in their external character as to render it exceedingly difficult, if not impossible, to distinguish them from each other, without subjecting them to chemical analysis.

I found the specimen first mentioned to be entirely and readily soluble in a solution of oxalic acid, at once proving the absence of lime, a distinguishing characteristic of natrolite.

Its quantitative analysis was as follows: Silicic acid, 47·626; alumina, 27·170; soda, 15·124; water, 9·780 = 99·700, and corresponds with the well-known formula of natrolite.

$$Na\ O\ Si\ O_3 + Al_2\ O_3,\ Si\ O_3 + 2\ HO.$$

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On comparing this analysis with those of the mineral already published, it will be found to approximate very closely to the most of them, and to agree with the calculated compositionof the foregoing formula.

Silicic acid,	2	atoms,	=	92·444	= 47·90
Alumina,	1	„	=	51·344	= 26·60
Soda,	1	„	=	31·178	= 16·
Water,	2	„	=	18·	= 9·22

Laumonite.—An analysis of Laumonite from Snizort in Skye has been already published by Connel; but that which I have examined is from Storr, in which locality it occurs in the form of a vein of from two to four inches in thickness traversing the trap-rock. It is associated with stilbite, and sometimes lies in immediate contact with it, having been supposed to be hypostilbite by some persons. Analysis, however, proved it to be laumonite, with the characters of which mineral it also agrees.

	Storr. By Scott.	Snizort. By Connel.
Silicic acid,	53·048	52·04
Alumina,	22·943	21·14
Lime,	9·676	10·62
Water,	14·639	14·92
	100·306	98·72

My analysis varies but slightly from that of Connel, the amount of silica and alumina being greater in mine. It may be remarked, however, that in his analysis there is a deficiency of about one-and-a-half per cent.

The formula which agrees best with the analysis of laumonite is that given by Gerhardt, although at the same time it must be admitted, that it is far from being satisfactory.

$$3\ Ca\ O\ 2\ Si\ O_3 + 3\ (Al_2\ O_3\ 2\ Si\ O_3) + 12\ HO,$$

its calculated constitution being, Silicic acid, 51·53; alumina, 21·49; lime, 11·92; water, 15·06; = 100.

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On the Erratic Formation of the Bernese Alps, and other parts of Switzerland. By CHARLES MACLAREN, Esq., F.R.S.E., F.G.S., and Member of the Geological Society of France. Communicated by the Author. With Map and engraved Illustrations.

The erratic blocks, or "travelled stones," of Switzerland have long afforded matter of speculation to geologists, though they are rarely noticed by the crowd of fashionable tourists who ramble over its mountains every summer. These blocks are extremely numerous, and present themselves in singular and unexpected situations; they are often of vast size, in some instances as large as a house; and they are occasionally found at the distance of fifty or a hundred miles from the parent rock. The mode of their transportation constitutes a problem about which volumes have been written, and which can scarcely yet be said to have received a satisfactory solution. Fifty years ago, the favourite theory was, that they had been forced along by currents of water. More recently, some have conjectured that they were floated on currents of mud; but at present, geologists may be divided into two categories, those who believe that the boulders were transported on rafts of floating ice, and those who hold that they were conveyed by glaciers of vast size, which had at one period covered all the low country. In the following notes, which are partly the result of two short tours in Switzerland, and partly derived from works written on the subject, originality or novelty of view has not been aimed at. My object has been to shew how the phenomena present themselves to a traveller pursuing some of the common routes, and to indicate how the facts are explained by the prevalent hypotheses.

Map I., representing a part of the Bernese Oberland, well known to tourists, is copied from the map of Keller.

T, the east end of the Lake of Thun.

B, the Lake of Brienz.

L, the Lake of Lungern, in Unterwalden.

Br, the Village of Brienz.

F F, the Faulhorn group of mountains.

M n, the Village of Meyringen.

G d, the Village of Grindelwald.

R, the Pass of the Grimsel, leading from the Valley of Hasli to the sources of the Rhone (o o) in the Valais.

r r′ r″, the upper part of the River Aar, which, after a course of 25 miles in the Valley of Hasli, flows through the Lakes of Brienz and Thun, and thence proceeds northward to the Rhine, of which it is the largest tributary.

J W N S V, the principal mass of the Bernese Oberland, comprising one of the most extensive groups of snow-clad mountains in the

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Alps, such as the Jungfrau (J), Wetterhorn (W), Engelhorn (N), Schneehorn (S), Viescherhorn (V). The south-east portion of the map, coloured red, is occupied by crystalline rocks, chiefly granite and gneiss. It is bounded by the line *l m n*. The district beyond that line on the north-west consists entirely of limestone. This well-marked boundary between the two species of rocks gives us a key to the origin and movements of the boulders. All crystalline blocks found in the limestone region have evidently been carried to a distance from their original site, which was on the south-east side of the line, *l m n*, or perhaps, in some few cases, at a more distant locality.

We begin our journey at Interlaken. This village, to which multitudes of English resort every summer, is of small size, but contains about a dozen of large hotels, where strangers are boarded and lodged at the low charge of five or six francs a head per day. It stands, as the name implies, between two lakes, those of Thun and Brienz, in a beautiful plain about two miles long and a mile and a half broad. It is scarcely possible to imagine a more lovely situation. With its trim verdant fields, its numerous orchards, its hedgerow trees, and its little pine groves, the plain looks like a patch of rich English landscape set down amidst the magnificent scenery of the Alps. Mountains of limestone, two thousand feet in height, rising up like gigantic walls, overlook and shelter it on the north and south, while the picturesque lakes of Thun and Brienz form its western and eastern boundaries. These mountains display great vertical faces of bare rock, separated by belts of pine or birch, or plots of fresh green herbage, rising one above another, and sometimes the covering of wood ascends to the top in dark unbroken masses. The traveller, as he rambles through the plain, comes unexpectedly upon hamlets and villages, which are hid amongst the trees; and though built of wood, and not over clean, the houses have the snugness and picturesque appearance common to Swiss cottages. The crowning feature of the scene is the Jungfrau, 13,700 feet in height, rising over the valley of Lauterbrunnen, and

> "Soaring *snow-clad* through its native sky,
> In the wild pomp of mountain majesty."

It is seen from every part of the straggling village, and owing probably to the transparency of the atmosphere, looks as if it could be reached in half an hour, though it is actually eleven miles distant in a direct line. The optical illusion producing these deceptive ideas of distance is common in the Alps.

Shortly after my arrival at Interlaken, wishing to get a view of the environs, I climbed to the top of a projecting rock on the north side of the river (at *a* in the map), a little westward of the wooden bridge. Here I stumbled upon half a dozen blocks of granite and gneiss in a situation where I little expected to find them. The

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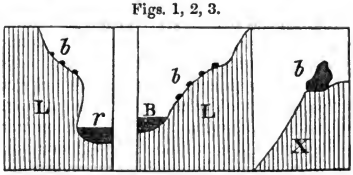
limestone rock (L in the small section, figure 1 below) is washed at the foot by the Aar (*r*), which it overhangs, and the surface on which the blocks (*b*) rest is extremely steep. To the upper ones, about 300 feet above the river, I could only ascend by using both hands and feet. Some of them contained one or two cubic yards of stone, and were in situations from which a slight force would precipitate them to the bottom. I was agreeably surprised, for I did not expect to meet with facts illustrative of the erratic formation so easily. The granite blocks came from *q*, or some place within the boundary line *l m n*—that is, they had travelled twenty miles or more. In this simple fact I had presumptive evidence that the agents which transported them were not currents of water; for currents strong enough to bring them hither would not have left them on a steep ledge of rock from which a slight impulse would detach them.

Figs. 1, 2, 3.



At *b* in the map, on the south side of the lake, near the village of Bonigen, there is a projecting mass of a conical form, which leans against the mountain, as if it were an outer portion of the rocks above, which, having lost its hold, had slid downward, and in sliding downward had been pushed outward. The hollow above it has some resemblance to a corry. This semi-cone is 600 or 700 feet in height; its base projects perhaps 1000 feet from the side of the mountain, and its circumference may be nearly a mile. Three depressions like terraces, one above another, are seen on its surface, which is everywhere covered with clay, earth, or gravel, though there is in all probability a nucleus of displaced rock below. Figure 2 above, is a section of the lowest of the three terraces; B, the lake of Brienz; L, the limestone of the hill, assumed to be the nucleus of the cone; *b*, blocks resting at various heights on its surface. Ascending by the east side, I met with a block of granite 3 yards long, 3 broad, and 1½ yards thick, measuring of course about 10 cubic yards, and weighing twenty tons. It was resting on the soil, 120 feet above the plain, on the side of a declivity dipping at 50° or 60° —so steep, indeed, that if it had fallen even from a height of 3 feet, it would infallibly have slid or rolled to the bottom. It had travelled 20 or 30 miles, but the agent which transported it must have set it down as cautiously as a nurse deposits a sleeping child in its cradle. Its position suggests the idea either that it must have been stranded here by floating ice, or that it and the soil under it

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formed part of the lateral moraine of a glacier, which was then subsiding under the effect of partial fusion.* The highest blocks I saw here were about 200 or 250 feet above the plain and the lake, but I have no doubt that there were others at a greater elevation. The upper ground was pretty carefully fenced, and I did not choose to become a trespasser.

A bank of loose materials extends from the west side of this cone along the foot of the mountain nearly to the opening of the valley of Lutchen at *d* in the map. It is from 50 to 200 feet in height and half a mile in length. The breaches made in it by transitory torrents rushing down from the hills above, shew that it is composed of gravel and sand or clay, and many erratics of granite or other crystalline rocks are lying on the surface. Figure 3 represents one of these, *b*, which is 12 feet high, 10 feet long, and 10 broad, and must weigh 50 or 60 tons. It stands on the edge of a very steep declivity of soil 80 feet in height, X. This long bank of sand and gravel with the boulders resting on it, is apparently an ancient moraine. While I was sketching the shape and position of the block, a man of rather uncouth appearance, with a large rusty sword at his side, came up. I attempted to get into conversation with him, but in vain. He knew no French, and I was deficient in colloquial German. I was only able to make out that he was a policeman; and when I pointed to "der gross stein," and manifested curiosity about its history, his reply was "icebergs—icebergs." The popular opinion here (if the policeman may be taken as its expositor) seems to be that the travelled stones were transported by floating ice.

Blocks of granite and gneiss are rare *in the plain* at Interlaken and elsewhere in Switzerland, but they may have existed formerly. They are the best building stone of the country, and their disappearance is supposed to be accounted for by the use made of them in the construction of bridges and other substantial works. The Swiss cottages are of wood with a foundation of stone a foot or two in height, for which any sort of rock suffices.

A small steamer carries the traveller to Tracht (*f*), near Brienz (Br in the map), at the east end of the lake. A sharp ridge of lime-

* Fragments of the rocks flanking a glacier valley, detatched by frost or avalanches of snow, fall down and collect on the two sides of the glacier, and are carried slowly downward with the ice. These are called "lateral moraines." In a compound glacier formed by the union of two tributary glaciers, the two inner lateral moraines unite and constitute a third longitudinal row of fragments resting on the middle, which is called a "medial moraine." One formed of three or four tributary glaciers has two or three medial moraines. And if such a glacier has its breadth much contracted in passing through a narrow gorge, the lateral and medial moraines are generally blended, and spread over the whole surface. These fragments, arriving at the lower end of the glacier, fall over and collect in front of it, and are called its "terminal moraine." When a glacier recedes it generally has several terminal moraines, one behind another, in the form of ridges of blocks with clay and sand.

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stone rises here to the height of 200 or 250 feet behind the Croix Blanche Hotel, and exceeds a furlong in length.

Figs. 4, 5.

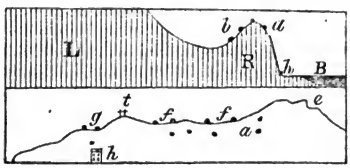

Figure 4 is a view of this ridge from the lake, *e* its east, and *g* its west end. The dots at *a* and under *f f* and *g* are travelled blocks resting on the top and sides; *h* is the hotel, and *t* a mimic wooden temple. Figure 5 is a section across the ridge. L, the foot of the limestone mountain, which rises to the height of 2000 feet; R, the ridge; *a* and *b*, boulders of granite or gneiss resting on the steep acclivities or the top; *h*, the position of the hotel; and B, the lake of Brienz. The boulders are of all sizes up to three yards in length, and the larger ones have their angles quite sharp. One of eight feet in length at *a* on the south side, rests on a surface so highly inclined, that if, when lodged here, it had been dropped from a height of two or three feet, it would certainly have descended to the bottom of the precipice. Many small blocks of granite may be seen built into a wall below *b*. They are generally rounded, and some of them were perhaps brought from the top or upper part. The west end of the ridge *g* is the broader, and is covered with soil as well as boulders; at the east end (*e*) the bare rock projects, and there is little soil and no boulders. It is the phenomenon of "Crag and Tail," so well known in Scotland, the crag appropriately facing the point from which the moving agent came. Agassiz informs us that when a projecting rock rises through a glacier and reaches its surface, or stands out a little above it, some of the large stones which strew the top of the glacier are stranded on the rock, and remain perched on its summit (*restent perchés sur la pointe de rocher*), or are deposited on its shelving sides, forming a ring or coronet round the summit. It will be seen how well this applies to the present case. Evidently no physical agent is so admirably adapted for placing boulders in these singular positions as a glacier. Gliding onwards in its irresistible course, with a motion so slow as to be inappreciable by the senses (one or two feet per day), the delicacy and steadiness of its pressure must be extreme. It is only by such an agent that we can conceive a mass of rock, weighing ten or twenty tons, to be lodged on a point or declivity, where it is so nicely balanced that the force of a man's hand would send it rolling to the bottom. It acts with the same delicacy

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in withdrawing as in applying its pressure. When the mild weather comes, the surface of the ice melts and evaporates, film by film, and the glacier subsiding *at the rate of one or two inches per day* (as shewn by Mr Charles Martins, in his researches on the Faulhorn glacier), withdraws its support, as it were, by grains or scruples, in a manner which even the most cautious hand could scarcely imitate. It is plain that a mass of floating ice loaded with stones could not act with the same nicety. Drifted, as it would be, by winds, tides, or currents, it would encounter fixed objects with a shock which might break it in pieces, and throw the stones it bore violently to the bottom. Or supposing an ice-floe, with a boulder resting on or frozen into it, to be stranded on a projecting rock like R, the boulder would not be deposited till it lost its hold by the partial fusion of the ice, and then its fall would be sudden and violent. I have enlarged on this point, because the two agents, floating-ice and glacier-ice, which have been called in hypothetically to explain the Erratic phenomena, have much in common in their mode of acting, and it is difficult to find characteristic facts to distinguish the agency of the one from that of the other. (See Charpentier, *Essai*, sect. 51.)

Opposite to Brienz, on the south side of the lake, is the Giessbach (*g* in the map), a famous waterfall which all travellers visit. It is a succession of cascades or cataracts, by which a large volume of water descends along a steep acclivity from a great height. It has cut a channel in the side of the mountain from 50 to 150 feet in depth, and to add to its picturesque beauty the thunders of the nearer cascades, blended with the echoes of the more distant ones, roll on the ear amidst a little forest of pines. I clambered up the acclivity to the height of about 300 feet, and found straggling blocks of granite, gneiss, or mica slate, as far as I ascended, either in the bed or on the sides of the torrent. Some of them were masses of six or eight cubic yards, and I noticed one about forty feet above the lake, whose situation, on the verge of a little precipice, again suggested the inquiry, what agency could bring it there without precipitating it into the water? Erratic blocks undoubtedly exist at many other points on the shores of the lake. Those mentioned fell in my way when I was merely visiting the localities usually visited by travellers.

In the preceding cases I had met with crystalline boulders at no greater elevation than 300 feet. My next excursion was to the mountain of Abendberg (*c* in the map), about two miles south-west from Interlaken. Dr Guggenbuhl, a benevolent German, has an establishment for cretins on this hill at the height of 1800 feet above the plain. In the ascent to it I found the boulders up to an elevation of 700 or 800 feet, beyond which the surface is so very steep that large ones could scarcely rest on it. I saw two blocks of gneiss or mica slate, the one four yards long, the other five, which had perhaps been originally united, and must then have constituted

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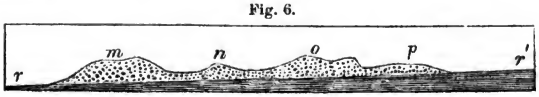
a mass of fifteen cubic yards. The blocks were generally angular, but some were rounded.

Rather more than a mile south-east from this locality, there is a great deposit of alluvial matter in the valley of Lutchen at *d*. The valley is less than half a mile in width, and is bounded by precipices of limestone rock, nearly vertical, and 1000 feet in height. The river runs along the west side of the valley, sometimes close to the rock, and the deposit is on the east side. The figure below is a section.

Fig. 6.



r r' A sloping line, representing the bed of the river, which rises gradually to the south.

m, *n*, *o*, *p*. The alluvial deposit, consisting of sand, gravel, and boulders, extending nearly a mile along the valley. Its interior has been laid bare by the stream, which, having cut a channel through it very near the western wall of the valley, has everywhere a *talus* of debris on its eastern bank. The height of the mass of debris, if taken a little behind the top of the *talus*, may be about 200 feet at *m*, 100 at *o*, and 50 at *n*, but it is three times greater near the eastern wall of the valley, and the mean depth of the whole mass from *m* to *p* will exceed 200 feet. A great proportion of the materials is composed of the debris and detritus of the adjacent limestone, but blocks of granite gneiss and mica slate, many of them measuring from 10 to 30 cubic yards, are distributed through it, and rest on the surface in thousands. Now these blocks must have travelled along the valley of the White Lutchen from *l*, or that of the Black Lutchen from *m*, a distance of ten or twelve miles. The origin of this mass is not difficult to explain. Its form and composition indicate that it consists of a series of terminal moraines, left at the end of the glacial period, by the glaciers of the Lutchen Valleys, during their gradual retreat from the low country to the remote recesses in the mountains which they now occupy. Much of the west side has no doubt been swept away by the river before it had excavated its present channel.

The reader must now accompany me to the little lake of Lungern (L in the map), north-east from Brienz. Setting out with my companion from Lucerne—a little town in a romantic situation, and commanding an unrivalled landscape—we proceeded by boat and carriage to Alpnach and Sarnen, and from Sarnen to the lake of Lungern. At this little wild lake we got on horseback, and crossed the Pass of the Brunig (*h*) by a breakneck road to Meyringen (*M*) in the valley of Hasli. The summit of the Pass is 1600 English feet above the lake of Lungern, and 1740 above the valley of Hasli. This is the lowest

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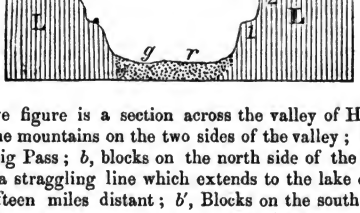
point in the crest of the ridge, which rises elsewhere to an elevation of nearly 3000 feet. Boulders of crystalline rock were thinly strewed all along the valleys of Sarnen and Lungern, up to the summit of the Pass. Having crossed it, we found them still more numerous on the south side of the crest, and of larger size (some, for instance, from 10 to 20 feet in length, and generally angular), affording evidence that the stream of blocks proceeded from south to north. The water, ice, or whatever carried the blocks and poured them into the valley of Lungern, must therefore have filled the valley of Hasli to a height little short of 2000 feet.

On the opposite side of the valley of Hasli (at *k* in the map), a large stream, coming from the south-west, pours over the limestone ridge, and constitutes the celebrated waterfall of the Reichenbach. Here also, on the verge of the precipice, the gneiss and granite blocks abound.

Fig. 7.



The above figure is a section across the valley of Hasli. L L, the limestone mountains on the two sides of the valley; R, the crest of the Brunig Pass; *b*, blocks on the north side of the Pass, forming part of a straggling line which extends to the lake of the Four Cantons, fifteen miles distant; *b'*, Blocks on the south side of the Pass, which are more numerous and more closely grouped; *k*, the opposite or south wall of the valley, at the point where the channel of the Reichenbach stream crosses it; *b''*, numerous boulders of crystalline rock, lying on the declivity up to the very brow. My impression is that the height of the rocks at *k* is about the same with those at R, but I have no ascertained measurement to rely on. While R, however, is the top of a narrow ridge, *k* is the lower end of a declivity which extends south-west five miles to the Scheideck Pass (*p* in the map), where it attains an elevation of 4400 feet above the valley of Hasli at Meyringen. Now, when I state that over all the five miles primary boulders occur, it must not be concluded that they came from the upper part of the valley of Hasli. They travelled by a different route. The two glaciers of Grindelwald giving birth to the two rivulets at W in the map, and the glacier of Rosenlaui at N, have their termination in the limestone ridge W N, but that ridge is narrow, and these glaciers have their origin in an extensive

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formation of gneiss behind it. We learn this from the map of M. Desor, the fellow-traveller of Agassiz, in his "Nouvelles Excursions et Sejours dans les Glaciers, 1845." These glaciers, or rather the much larger ones which occupied their places at some remote period, must have been the agents which carried the primary boulders across the limestone ridge, and distributed them over a great part of the space from the Wengern Alp, *x*, to a point near *k*. On the east side of the lower glacier of Grindelwald, 100 yards from the ice, I found a block of gneiss measuring 35 feet by 20, and 12 in thickness, containing 300 cubic yards, and weighing 600 tons. It was most delicately poised on a steep declivity of soil, 800 feet above the rivulet, and well exemplified how nicely the glacier regulates its force in depositing the load it bears on its banks. There were others near it.

The valley extending from Alpnach to Lungern has features of great beauty. The mountains are high, and feathered with wood to their summits, while their declivities abound in groves and glades of the freshest green, and lower down are a few cultivated fields, chalets, villages, and the two sweet lakes. In looking northward the eye rests on the giant forms of the Rigi and Mount Pilatus, the one rising 4480, the other 5570 feet above the lake which bathes their feet. The day, unluckily for us, was wet, and we began the ascent of the Brunig in a heavy rain. But it abated greatly before we reached the summit, and we anticipated a delightful view of the beautiful valley of Hasli, and the mountains beyond it. When we emerged from the wood, however, and looked southward, the valley and the mountains had disappeared, and there was nothing before us but a vast expanse of snow-white clouds, above which we stood "like shipwrecked mariners on desert coast." Such a scene has a touch of the sublime. There is a mystical charm in the feeling of intense loneliness suddenly awakened in the traveller's mind when an image of chaos is thus conjured up around him, akin to what Noah may have felt in the ark when casting his eye over the boundless waste of waters; and the illusion is the greater if the traveller is in a strange country. But our chaos was not of long duration. In a little while the Ollchihorn reared its head right in front of us, and was followed by other "horns" and peaks, rising slowly from the ocean of cloud, like rocks and castles emerging from the canvas in dissolving views, till we had before us an archipelago of islands. After the mass of vapour rolled away from the mountain tops, it settled on the two sides of the great valley of Hasli (the bottom of which we had now reached) leaving the middle clear. Here it clung to the rocks like a festooned curtain, affording us, through rents and openings in its upper parts, many delicious little fairy landscapes, pine woods, precipices, waterfalls, bright green lawns, all placed in a setting of white cloud, and suspended high over our heads, as if belonging to an upper world. The scenery of the Alps has many phases, and those

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who have not seen them in shower as well as sunshine, lose one-half of their grandeur and beauty.

Figure 7 above, conveys an idea of the usual form of the lower valley of Hasli (Nieder Hasli) in cross section. The bottom, about three-fourths of a mile in breadth, is level, or a little raised in the middle, and entirely composed of water-worn gravel or sand, *g r*. At one or both sides there is generally a vertical precipice of limestone, one, two, or three hundred feet high, with a talus of debris at its foot, the wrecks probably of a lateral moraine. On the top of the vertical precipice at 1, is generally a sloping shelf covered with bright green herbage or shrubs. Behind this is a second precipice, also vertical or nearly so, and crowned with a second grass plot, 2. Above this is a third, and even fourth precipice, but the upper rocky surfaces slope backward more rapidly. By such steps the wall of the valley rises to a height of 2000 or even 3000 feet, with patches of *grass* occurring at intervals, up to the line of perpetual snow. We saw no heather in the Alps.

At the sites *a*, *b*, *Br*, and *g* in the map, the travelled blocks were met with only 200 or 300 feet above the bottom of the valley, but if I had been able to search the ground high above, there is little doubt that I would have found them at as great an elevation as at *h* or *k*—that is, 1700 or 1800 feet. Nor is this the extreme height they have attained.

A little above Meyringen the valley of the Aar is barred by a ridge of limestone of considerable height, through which the river has cut a very narrow channel. The top of this ridge is much smoothed, and at one place very distinctly striated. A considerable number of granite and gneiss boulders were resting on the top, the large ones generally angular, the small ones rounded, but on the south face of the ridge which looks up the valley they were lying in hundreds. Granite has a great economic value in Switzerland, and many of these blocks, I was told, were carried to Berne some years ago, a distance of fifty miles, to serve as building materials for the new bridge. But, independently of the boulders, the ridge itself is a curious object. It is nearly a mile in length, measured *along* the valley, and 500 feet in height; and being apparently of the same rock with that which bounds the valley above and below, the question suggests itself how the agent, whatever it was, which scooped out the rest of the channel to so great a depth, and gave it a slope of such unbroken uniformity (of probably 1 foot in 200 or 300), should have suspended its work here, and left this mound of rock in its place? It seems probable that the two lateral valleys *y* and *z*, which join the main valley here, had some connection with the existence of the mound in question. If each of these had its glacier, the two lateral glaciers must have modified the action of the principal one. They might obstruct its downward march, by pushing forward masses of debris, or they might raise a solid barrier of ice like that which shut the

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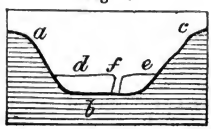
valley of Bagnes in 1818, in the form of a dike, 500 feet high. (Agassiz, *Etudes*, p. 156.) In either case, the glacier of the Aar would be be arrested in its downward course, or else compelled to mount above and override the barrier, and in this way the rock immediately behind the barrier would be protected from a large share of the denudation the parts of the valley above and below must have undergone. If this view be adopted, the Kirchet may be considered as a portion of a *more ancient* bottom of the valley, and it may be inferred that the deeper channel on both sides of it, which descends nearly 500 feet lower, has been excavated by the glacier of the Aar.

In the annexed figure the heavy line *a b c*, is a cross section of the valley of Hasli, either above or below the Kirchet; *d f e*, the Kirchet, stretching across the valley, whose section at this point is *a d f e c;* and *f* is the fissure, narrow and sinuous, in which the river Aar now flows.

Fig. 8.



This fissure, undoubtedly cut by the river itself, indicates both the mode and the extent of its excavating power, and in contrast with it the larger opening *a b c*, shews the vastly greater power of the ancient agent—the glacier.

The Kirchet is the boundary between the upper and lower valleys of Hasli (Ober and Nieder Hasli.) About two miles above it there is an ancient terminal moraine, consisting of vast piles of boulders chiefly of granite and gneiss, mixed with soil, and probably 200 feet in height. It had formed a bank across the valley, but a deep passage has been cut through it by the river. A little beyond this, the limestone is succeeded by gneiss and mica slate, and about two miles farther up at Guttanen, the gneiss is succeeded by granite, which continues to the Grimsel, a distance of eight miles. (*Studer, Geologie der Schweiz*, p. 178).* From the point where the crystalline rocks commence the valley contracts, the flat bottom, so striking a feature lower down, disappears, the ascent becomes much more rapid, the road perilous, and the scenery wild. Striated and grooved rocks, so rare in the limestone district, are now seen everywhere. The "hospice," or hotel of the Grimsel (at R in the map), stands amidst savage rocks, at an elevation of 6158 feet above the sea, and close to the Pass which connects the valley of Hasli with that of the Rhone,

* The first volume of the *Geologie der Schweiz* was published in 1851, in 12mo, with a small map of the Alps and Northern Appenines. The second, which will complete the work, is expected to appear before the end of the present year, and will be accompanied by a map on a larger scale, carefully coloured. A Manual of the Geology of Switzerland from the hands of the eminent Bernese Professor, will be regarded as a great acquisition by all who cultivate the science.

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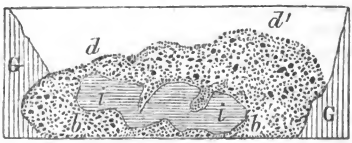
o o. In this dismal and desolate region, with much snow still on the ground (24th July), we had comfortable meals, served up by the landlord's daughters, three respectable young women, with the appearance and manners of ladies. Our fellow-lodgers numbered about forty, and though the building seems low and small, and is as rude as the scene around it, the whole were supplied with beds. It is wonderful how much is done for the traveller's comfort in Switzerland. As a specimen of the climate, Mr Martins informs us that in the six months from November 1845 to April 1846, no less than fifty feet of snow (fifty-three feet English), equivalent to fifty-five inches of water, fell at the Grimsel.

The head of the valley of Hasli is about three miles west of the hotel. Here I found the mighty agent whose operations I had traced over a line of forty miles, still at work, though sadly shrunk from what must have been his pristine dimensions. The large glacier of the Unter-Aar was before us, and the figure below is a front view of its lower end, taken from the left side, and foreshortened in the horizontal direction.

Fig. 9.



G G, the two walls or sides of the valley, formed of grey granite which rise abruptly to a height of 1000 or 1500 feet, at an angle of 50° or 60°.

d d', The top, as seen from below, covered with blocks and fragments of various sizes. The same materials strew the whole front down to *b b*, except the space marked *i i*, where the ice is seen in very distinct strata, averaging, probably, about two yards in thickness. It has been shewn by Professor Forbes that what seem "strata" in a glacier, are curved laminæ of a conoidal form generated by the unequal motion of the ice—the middle moving faster than the sides, and the top faster than the bottom. The left or north side of the glacier *d'*, is higher than the right *d*, and fully 300 feet above the gravelly bed of the streams at the foot of the glacier. Its breadth from G to G in M. Desor's plan (1842) is about 1600 feet, or nearly one third of a mile; but the glacier being formed by the junction of two others (those of the Finster and Lauter Aar), is five miles long if measured back to the point where they unite, and at the higher end has a breadth of 4000 feet. The glacier of the Finster Aar was sounded by M. Desor to the depth of 761 feet, without being sure that he had reached the bottom. The part of the frontal

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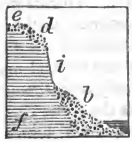
declivity *i*, consisting of ice, was nearly vertical, as in the annexed section; while the part between *d* and *b* (Fig. 9) on the north side was entirely covered with blocks, and inclined probably at an angle of 30° or 35°. Owing to the mobility of the blocks affording very insecure footing, we ascended by the side of the fixed rock and partly upon it, and walked about a mile along the surface in the direction *d e*. Its appearance was new and strange, quite unlike anything I had previously seen on glaciers. No ice was visible, no groups of picturesque cones like spires, none of the huge transverse rents called crevasses; from side to side the surface was a sheet of fragments great and small—resembling a dry river channel covered with stones, and confined by walls of rock above 1000 feet in height. Yet the coating of stones, though massive in appearance, was really thin; for, on shoving aside two or three of the smaller fragments, the ice generally came into view, and no doubt constituted the entire mass from *e* to *f*. The debris spread over the surface in this way, forming what Charpentier terms the "superficial moraine," are all carried ultimately, by the slow progressive movement of the glacier, to its lower end, where they drop over the declivity, and, resting at its foot, are called the "terminal moraine" (*b*). The fusion at the lower end prevents this progressive motion from adding to the glacier's length.



The glacier, by means of the mud, sand, and gravel which lie below it, or adhere to its under part, polishes, scratches, and grooves the rock in contact with its sides and bottom. The scratches and grooves correspond with the line of the glacier's motion—that is, they are horizontal, or nearly so, even upon vertical surfaces, and their aspect, form, and direction, with the polishing which accompanies them, are so peculiar and characteristic, that, when they are found in any valley now never visited by permanent ice or snow, they afford decisive evidence of the former existence of glaciers at the place; for they are such as no other agent known in nature produces in such localities. The limestone of the Alps, at least that of the valley of Hasli, wastes too rapidly to retain the scratches and grooves, unless where it is well covered with soil, but they present themselves in abundance as soon as we enter the region of granite and gneiss. Now, these characteristic marks of glacier action are met with, not only in the neighbourhood of the present glacier, and on the same level with it, but ten miles lower down in the valley, and more than 1000 feet above it in vertical height. Agassiz describes the grooves in the valley of Ober-Hasli as from an inch to a foot in breadth. There are many of them, however, two feet in breadth, some even three or four, and this on surfaces of rock almost vertical, and 1500 feet above the traveller's head. They exist in thousands, and are so conspicuous that, in wet weather, their glittering aspect

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constantly attracts the eye. Between the hospice of the Grimsel and the present glacier there are several places where you may find a precipitous face of rock, of the extent of an acre, all grooved with broad shallow horizontal grooves, but marked off into oblong spaces by dark, vertical, and horizontal lines, caused by fissures in the rock. As the granite here is in strata, resting on their edges, according to M. Desor, the vertical fissures must be *seams*, and the horizontal ones *joints*. When the surface of the broad grooves is sufficiently near the eye to be examined, it presents fine striæ. These may be seen at the large smoothed area, called the Hellenplatz (figured in Agassiz's 16th Plate), a little above the Handeck Waterfall, and with the aid of a pocket telescope even a better view may be got, both of the grooves and striæ on a rock on the opposite side of the river.

Holding it then as established, that distinctly-grooved rocks in Alpine valleys are sure marks of the action of glaciers, the next question is, To what altitude above the present bottom of the valley of Hasli are these marks found? Now, on this point we have a distinct statement from Agassiz. He ascertained by barometrical measurement, that on the Siedelhorn, which flanks the glacier on the south, the striated rocks rise to the height of 2762 feet (English) above the valley at the foot of the existing glacier. (*Etudes*, p. 254.) There are estimates even higher than this, for Elie de Beaumont admits that the upper limits of the erratic zone (marked by polished rocks or travelled boulders) in the Alpine valleys indicate a depth of 800 or 1000 metres.—(*Remarques sur deux points de la Théorie de Glaciers*, 1842.) We are safe then in assuming that a glacier 2500 feet or more in depth occupied the valley of Hasli at an ancient period. With such a depth we can well understand that the river of ice might transport blocks of granite across the Brunig Pass into the valley of Sarnen, which had then of course a glacier of its own, with a northerly motion. The glacier of the Aar had then occupied the whole of the Alpine portion of the valley, fifty miles in length, which ends at Thun, and must have extended beyond it into the plain, as far as Berne, where the remnants of a moraine still exist. What the *minimum* of inclination is necessary to give motion to a glacier will be afterwards considered. From the lower end of the present glacier to Brienz the fall is one foot in thirty-four, or an inclination rather under two degrees; from the same point to Thun, it is one foot in sixty-four, or fifty-three minutes.

The sum of the argument derived from the preceding details may be thus stated:—

There are two facts characteristic of existing glaciers in the Alps. *First*, They carry down from the higher parts of the valleys masses of rock often of vast size, and deposit them on the sides of the lower parts of these valleys, or at their terminations. *Secondly*, They polish and striate the rocks in contact with them. Now, in the valley of Hasli, which we have been examining, we find a glacier at

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the upper extremity performing these functions—transporting boulders, and polishing and striating the rocks in contact with it to the height of 300 feet above the visible bottom of the valley. But in the same valley, both at the present glacier and many miles lower down, we find the same characteristic marks of glacier action; at a much greater elevation, we find large boulders, and these generally *angular*, not rounded or water-worn, transported from the upper valley and lodged on the sides of the lower at a height not much short of 2000 feet, and we find polished and striated rocks at the same elevation. Can we doubt that the same effects in both cases proceeded from the same causes—that the agent which now deposits boulders and polishes rocks at 300 feet of elevation, also deposited the boulders and polished the rocks at 1700 or 2500 feet?—in a word, that a glacier 2500 feet in depth at some former period occupied the valley of Hasli, and extended to Thun, or beyond it? Where the parallelism is so complete, it would be against all sound philosophical principles to account for the phenomena by calling in a different agency, and one, too, purely hypothetical, to supersede that which is in operation before our eyes.

Transportation of Alpine Blocks to Jura.

We have thus good evidence that glaciers like the present, but of much greater dimensions, afford a satisfactory explanation of the transference of granite blocks from the higher Alps to the lower ends of the valleys in the limestone district—that is, to the borders of the level country. It remains to be considered how far the same agency will account for the transportation of blocks from the Alps across the level country to Mount Jura. These blocks were long a puzzle to geologists, and are still a marvel to tourists. They are of granite gneiss, and other rocks belonging to the Alps, and they are seen lying in thousands on the southern face of the limestone chain of Jura, to which they must have been carried across the plain of Switzerland over a space of fifty miles or more. They are found not merely at the foot, or on the lower declivities of Jura, but high on its sides, at an elevation of 2000 feet above the country they had traversed. The first hint of the theory which attributes the conveyance of the granite boulders to glaciers was given by our townsman the late Professor Playfair. It was afterwards broached by M. Venetz, a Swiss engineer, who probably was not aware that his idea had been anticipated. It was next adopted by Charpentier, who fortified it with a great variety of evidence in a memoir produced in 1834, and republished in an enlarged form in 1840; and it has received further support from Agassiz and Guyot of Neuchatel.

Map II. represents the western portion of Switzerland.

G, the Lake, and *g*, the Town of Geneva.

B D, the Bernese Oberland. The Lakes of Thun and Brienz are seen near D.

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N, the Lake of Neuchatel.

B *l*, Mont Blanc.

t' t, the Pennine Chain which bounds the Great Valley on the south.

v v, the opposite chain which divides the Bernese Oberland from the Great Valley.

r a c q d u p n y b, the dotted space thus marked is the Valley of the Rhone (which, for distinction sake, I shall call the Great Valley.) It is shut in on all sides by high mountains, except at *e*, where the river escapes through a broad and deep opening. The true breadth of the Great Valley is much greater than that shewn in the map.

g f x h i k, the western part of the Plain of Switzerland, and the southern declivities of Mount Jura, over all which erratics, consisting of blocks of granite, gneiss, serpentine, &c., are distributed, which have been proved to be derived from the Great Valley above mentioned. The area over which the transported materials are spread extends from Mount Sion (*S*), on the south-west to a point near Soleure (*h*), on the east. Its length is about 110 miles, its breadth from *k* to *f* 30, and the blocks ascend on the side of Mount Jura at *f*, to a height above the plain which has been variously stated, but which, on the authority of Elie de Beaumont, I put down at 3450 English feet (1050 metres) above the sea, or 2015 above the lake of Neuchatel.

It is by means of certain rocks of a marked lithological character, and therefore easily recognisable by a good mineralogist, that the travelled boulders strewed over Jura and the Plain of Switzerland have been traced to their primitive sites, and the course they pursued in their migrations ascertained. The phenomena are much more complicated here than in the Valley of Hasli, and also on a much grander scale. The Great Valley, *r a c q*, &c., is 100 miles long and 50 broad, and every part of it has furnished its contingent of blocks and fragments. M. Guyot deduces from an elaborate investigation of the phenomena, that the boulders are not scattered promiscuously over Mount Jura and the Swiss plain, but that a certain order prevails in their distribution, similar to that which prevails among the materials brought down by glaciers, in the shape of lateral, medial, and terminal moraines. (*Bulletin de la Societé des Sciences Naturelles de Neuchatel*, Seances Mai, Nov., and Dec. 1845.) The travelled masses relied on as evidence are, with one exception, all igneous or metamorphic—namely, granite of three varieties, gneiss, chlorite slate, euphotide eclogite, serpentine, and a peculiar conglomerate. These being spread over a district (*g f h i k*), composed of rocks entirely different (sandstone and limestone), are easily discriminated. And even the precise locality from which a block came can in some cases be ascertained.

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The boulders attain their greatest elevation on Jura, at the hill of Chasseron (*f* in the map), precisely opposite the valley (*e*), through which they must have passed. At this spot, granite blocks from the east shoulder of Mont Blanc (*u*) are found 2000 feet above the plain. From *f*, the boulders continue to present themselves on the declivities eastward, but at lower and lower elevations, all the way to *h*, where they descend to the level of the plain. They continue also westward to *g*, with a similar change in elevation, so that, if a section were made along the south face of Jura, it would form an arc, of which the middle would be probably 1500 feet higher then the extremities. Supposing the fact to be well ascertained, Charpentier justly considers it as strong evidence to shew that the boulders were transported by glaciers. For, in this case, the primary movement of the ice (a semifluid mass, be it remembered) would be in the direction of the valley (*e*) from which it issued—that is, right to *f*. It would indeed tend to diffuse itself laterally as soon as it reached the low country, but the dynamic pressure, so far as it acted on the mass, would operate most strongly in the direction *e f*. The progress of the glacier would be stopped by the mountain at *f*, which would then become a secondary centre of dynamic pressure, and accelerate the motion towards *g* and *h*; and the semifluid ice thus acted on, and obeying the law of gravitation, would *have its surface gently inclined to these two points*. The boulders resting on Jura would thus be deposited at lower and lower levels, in proportion as the places they rested on were farther and farther from *f*. If, on the contrary, we assume that the boulders were transported on floating ice, is it not evident that they would have been found at as high a level at *g* and *h* as at *f*? for water, unlike a viscous fluid, would find a true level over a space like this in a few hours or days, while the great number of the boulders shew that their distribution must have extended over thousands of years. (Charpentier, *Essai*, sect. 53.)

M. Guyot, states, that on the declivities of Jura, on the Bernese Oberland, from *k* to *i*, and even in the intermediate plain, a *linear arrangement* of the blocks and fragments may be traced, such as exists in moraines, and a distribution in harmony with the laws of glacier motion. Thus, fragments derived from the right bank of the Great Valley (*a c q*), are found on the right side of the basin (*k i*), while those derived from the left bank (*y n p u*), are found on the left side of the basin (*f* and towards *h*), and those derived from the upper left bank (*b t*), occupy the middle of the basin, and are found in the space between *i* and *h*, where they constitute what M. Guyot calls the frontal or terminal moraine of the eastern glacier. There is a huge boulder of talcose granite resting on a low eminence at Steinhoff, ten miles south-east from Seleure (near *h*), containing 61,000 cubic feet (French), and weighing more than 5000 tons. It will give a better idea of its magnitude to state that it is equal in bulk to a mass measuring forty feet in length, breadth, and depth—as large, in short,

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as a goodly house of three storeys! Now, M. Charpentier ascertained from its composition, that the boulder is a portion of a rock on the south-east part of the valley (near *t*). From this point it had travelled round by the valley, *e*, (for there is no other outlet), *performing a journey of* 150 *miles*, to *h*, where it now lies, a monument of the vast powers which nature has at her command, and of the mighty physical changes which this part of Europe has undergone.

We have further evidence of glacier agency in the fact that the blocks and debris on the two sides of the Rhone are kept wonderfully distinct. The valley of Entremont, for example, at *p*, and the valleys of St Nicolas and Saas between *b* and *n*, bring down from the Pennine chain or southern ridge (*t' t*), thousands of fragments great and small, of the rocks pecular to each, talcose granite, chloritic gneiss, diallage, and serpentine. These abound along the left side of the valley, and are diffused far and wide over the Swiss plain; but what on a first view seems strange, none of them have been carried across the valley to *c* or *q*. Those from *b*, for instance, are found high on the slopes of Jura at *f*, a hundred miles from their parent rock, while they are not found on the ridge (*v v*) just opposite. Now, a similar law of distribution is peculiar to a glacier. The debris which collects on its right and left flanks, forming its lateral moraines, may be carried many miles downwards; and yet (unless they are forced together at a narrow gorge) not a block will pass from the one side to the other, though the space which divides them may be small. Had the blocks been transported on masses of floating ice, and acted upon, of course, by winds and currents, this remarkable separation could not have been maintained.

While the larger portion of the fragments derived from the Great Valley went eastward, and were diffused over the space *k f h i*, another portion, and at a later period, as M. Guyot thinks, went in the opposite direction, and formed the western glacier, whose course was through the Lake of Geneva (G), to the western part of Jura. Its right moraine was midway between *f* and *g*; its left ran along the south shore of the lake, and its frontal or terminal moraine was at *S*. To me, however, it appears that the whole ridge of Jura from *S* to *h* should be considered as part of the terminal moraine.

The blocks on the middle region of Jura, from *f* westward, and above the Lake of Neuchatel (N), from two zones. The upper reaches an elevation of 2000 feet; the elevation of the lower is from 500 to 1000 feet; and between them is a zone 1000 feet broad, nearly *destitute of blocks*. The granites of Mont Blanc from *u*, and the various rocks of the Pennine chain from *t* to *p*, constitute the higher zone almost exclusively. They are found in the lower zone also, but mixed with others. Though Jura is the extreme boundary of the erratic basin, the blocks are much more numerous on it than in the plain—a fact quite in harmony with the glacier hypothesis, for the plain would only receive the blocks which happened to be

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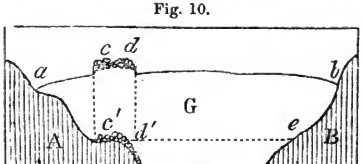
spread over the surface of the glacier at the moment of its dissolution, while Jura being the cite of a frontal moraine, would be a *landing place* for blocks, perhaps for thousands of years. (Charpentier, *Essai*, p. 267.) Had they been borne on floating-ice the order of distribution would have been reversed; they would have been most abundant near the source of supply, that is about G *k*, scarcer at *m o*, and very scarce on Mount Jura.

The Steinhoff boulder, containing 61,000 cubic feet, has been mentioned, but there are some others worthy of special notice. One of the most celebrated lies on Mount Jura at *x*, some hundred feet above Neuchatel (N), and being of easy access, has been visited by crowds of tourists. It is called *Pierre a Bot* (or toadstone), and measures 50 feet in length, 20 in breadth, and 40 in height. This great block is of granite from the north-east shoulder of Mont Blanc (*u*), and has been carried to a distance of 80 miles from the parent rock. I was prevented by accident from seeing the Pierre a Bot, but I saw many of the smaller size in the vicinity.

At Orsieres, near Martigny, there is a granite boulder estimated to contain 100,000 cubic feet, and weighing consequently 8000 tons. It is a travelled block, for it rests on a limestone mountain, but it probably has not travelled far.

At Monthey (near *e*) there is a remarkable group of granite blocks, amidst which I spent some hours. They lie on a sort of terrace, about 400 feet above the bottom of the valley, and form a belt from 300 to 800 feet in breadth, according to M. Charpentier, and a mile and a half in length. One, called *Pierre des Marmettes*, with a summer-house on its top, is 63 feet long, 32 broad, and 30 in height. Another, named *Pierre a Mourguets*, is 65 feet long. There are many others whose solid contents are from 300 to 400 cubic yards. The large ones have their angles almost always sharp, shewing that they have not been rolled or exposed to attrition, and this holds true of the travelled boulders on Jura and in the Alpine valleys generally. All the large blocks at Monthey are of one species, and belong to the granite of the north-east shoulder of Mont Blanc (near *u*), from which they are now 27 miles distant. Charpentier explains their depositation here by a glacier as follows:—

Fig. 10.



G, Cross section of an ancient glacier occupying the valley (at *e*, in the map) between the mountains, A B.

x 2

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a b, The upper surface of the glacier.

c d, A pile of blocks forming part of a *medial* moraine, resting on the surface of the glacier, but a little raised above it. The covering of stones protects the ice below, while the uncovered part being exposed to fusion and evaporation, wastes away, and the moraine is thus found riding on a ridge of ice, which Professor Forbes informs us is sometimes 80 feet high.

When the glacier was in progress towards final dissolution, its surface *a b* gradually subsiding, would arrive in course of time at the line *e*, and the blocks *c' d'* would then be deposited on the terrace in the position *c d* where we find them, 400 feet above the bottom of the valley, except a few which slid over the declivity. This explanation appears to me satisfactory, though our distinguished countryman, Sir Roderick Murchison, has raised some objections to it. We learn from Elie de Beaumont's Memoir that there are glacial traces on the hills near Monthey, at an elevation of 2350 feet (English) above the present bottom of the valley. The left *lateral* moraine, therefore, of the great glacier would be at *a*, probably 2000 or 3000 feet westward from *c d*.

Having sketched the distribution of the travelled blocks, we recur to the grand question—*What were the means of their transportation?* And as we found that Charpentier's theory affords a plausible explanation of the erratic phenomena in the valley of the Aar, let us inquire whether it is applicable to those we have been describing. The inquiry then presents itself in this form—whether the magnitude and position of the ancient glacier which occupied the Great Valley of the Upper Rhone, were such as, in accordance with the laws of glacier motion, would enable it to transport the Alpine blocks from their primitive sites to the Swiss plain, and the declivities of Mount Jura?

The data for the solution of this problem are not so ample as might be desired. The most important, so far as my information extends, are supplied by the Memoir of Elie de Beaumont previously referred to, in which he gives *the greatest elevation at which polished rocks and erratics* have been found at several points in the Great Valley and across the Swiss plain to Jura. These traces indicate the height and depth of the ancient glacier, and, when connected by measuring the intervening spaces, enable us to deduce the *slope* or inclination, which determined its progressive motion. The following is Elie de Beaumont's table, with the metres converted into English feet:—

Upper limit of Polished Rocks and Erratics above the Sea.

	English feet.
1. Near the Grimsel (*r* in the map), ...	7547
2. Near Aernen (between *r* and *b*), ...	5848

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	English feet.
3. Near Brieg (*b*),	4988
4. Near Martigny (*d*),	4757
5. Near Great Saint Bernard (on ridge *t'* above *p*),	8203
6. Mountain of Plan-y-Beuf (*p*),	5804
7. Above Monthey (*e*),	3796
8. Chalets of Playau (near *k*),	4010
9. Chasseron on Mount Jura (*f*),	3444

The traces at Nos. 5 and 6 belong to tributary or confluent glaciers. Setting these aside, and putting the spaces from 1 to 2 and 2 to 3 together, the slope or inclination, from the head *r*, of the ancient glacier, to its north-west termination *f*, is given by M. de Beaumont as follows:—

	Slope or Inclination. In degrees and minutes.	In feet.
From the Grimsel to Brieg (*r* to *b*),	1° 5′	1 foot in 51
... Breig to Martigny (*b* to *d*),	3′	1 ... 1143
... Martigny to Playau (*d* to *k*),	18′	1 ... 193
... Playau to Chasseron (*k* to *f*),	12′	1 ... 285

Leaving out Monthey, there is a constant *fall*, as the table shews, from one station to another, but at rates generally varying. From the Grimsel to Brieg it is 1 foot in 51. From Brieg to Martigny it is extremely small; in the two following intervals it is greater, though still below the first.

Dividing the whole space, 132 miles in length, into two sections, the inclination is:—

From the Grimsel to Martigny (*r* to *d*),	24′	1 foot in 143
... Martigny to Chasseron (*d* to *f*),	15′	1 ... 230

Or taking the whole in one continuous line:—

From the Grimsel to Chasseron (*r* to *f*),	28′	1 foot in 160

But the glaciers which occupied the valleys at Plan-y-Beuf, and of the Val de Bagne, at *t' p* and *u*, must not be overlooked. They are lateral valleys indeed, but their size and elevation, and the position they occupied, nearly in a line with the opening *e*, through which the grand glacier debouched into the plain, must have rendered them powerful auxiliaries. Descending from a great elevation along a steep surface, falling perhaps 1 foot in 12, it is probable, that, instead of joining the principal glacier laterally, they would override it, and increase its height by many hundred feet. We know, in fact, that it was from the valley of Ferrex, on the west side of Plan-y-Beuf, that a large proportion of the highest blocks on Jura (those at Chasseron) came. M. de Beaumont has accordingly recognised

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the importance of those glaciers, and calculated the inclination of the line connecting them with Jura, which is as follows :—

	Minutes.	In feet.
From Plan-y-Beuf to Chasseron (*p* to *f*),	22′	1 foot in 156
... St Bernard to Chasseron (*t*′ to *f*),	40′	1 ... 86

The data, however, on which these calculations rest are open to some objections. When the glacial traces consist of polished rocks, which are seldom continuous over great spaces, it may happen *that the highest have escaped notice.* Thus M. de Beaumont puts down 2300 metres as the upper limit near the Grimsel Pass ; but Agassiz and Desor subsequently found polished rocks on the mountain which forms the western side of that Pass, the Siedelhorn, at 2447 metres of elevation (Desor "*Excursions et Séjours*," p. 242.) Thus 482 feet were added to the difference of level between *r* and *f*, and the general slope was raised from 1 foot in 160 to 1 foot in 153. Again, when the difference of level between the traces at Brieg and Martigny (*b* and *d*), a distance of fifty miles, was put down at 70 metres or 229 feet, should not some allowance be made for the effect of the great glaciers which descended from the lateral valleys of Saas, St Nicholas, Annivier, and Erin (at *n* and *y*), in enlarging the principal glacier and forcing it to *expand upward* at points below Brieg. In the next place, glacial traces *may once have existed at a greater elevation, and been subsequently obliterated.* None are marked in M. de Beaumont's table as occurring in that long space of fifty miles, but as the rocks on both sides are of limestone, which wastes rapidly, few polished or striated surfaces could be expected. Moraines, indeed, may exist, though they also are liable to obliteration. On the whole, we cannot be sure that the traces now visible at any locality are the *highest* which have existed.

Depth of the Ancient Glacier.

The enormous depth of the ancient glaciers is still more astonishing than their length or breadth, and to this element we can approximate with the aid of Keller's map, which gives the height not only of the mountain tops, but of sundry points in the bottom of the valleys. Thus the elevation of the upper limit of erratics at Aernen (between *r* and *b*) is 1813 metres above the sea, in the table, and that of the town standing at the bottom of the valley, according to Keller's map, is 2990 French feet. Converting the measures into our own, and deducting the latter from the former, the depth of the glacier is found to be 2756 English feet. The whole calculated in this way are as follow :—

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	Depth or thickness of glacier.
At Aernen, ...	2756 feet.
... Brieg (*b*), ...	2662 ...
... Martigny (*d*), ...	2177 ...
... Monthey (*e*), ...	2356 ...
... Playau (*k*), assuming with Charpentier that the Lake of Geneva was covered with ice, on which the glacier floated, . .	2780 ...

If the glacier filled the bed of the lake to the bottom, its depth opposite the Rhone would be about 3200 feet.

Breadth of the Glacier.

When the glacier of the Rhone had a depth of 3000 feet, its breadth would probably not exceed 8 or 10 miles. It will be seen in the map that the Pennine Chain *t' t* throws out transverse ridges or "spurs," separating the valleys *p n y b*, each of which would have its distinct glacier, but all of them tributary to the grand glacier. These ridges terminate northward in peaks rising 5000 or 6000 feet above the bottom of the valley, and consequently 2000 or 3000 above the top of the grand glacier. The space between these peaks and the northern chain *v v*, which defines the breadth of the grand glacier, varies from 10 to 14 miles, but would not exceed 8 or 10 at the surface of the ice. But after escaping from the valley of the Rhone into the Swiss plain, its breadth would dilate to 30 miles, if reckoned from *k* to *f*, or to 110 if reckoned from *S* to *h*. The dimensions of the ancient glacier which spread the debris of the Alps over the plain of Switzerland and the declivities of Mount Jura, may therefore be estimated approximately as follows :—

Length from *r* to Mount Jura at *f*, 132 miles; from *r* to north-east terminal moraine at *h*, 160 miles; and to the south-west one at *S*, nearly the same. *Breadth* in the Great Valley from 8 to 10 miles, in the Swiss Plain 30 miles in one direction, and 110 in another. *Depth* in the Great Valley from 2600 to 3000 feet, near *k* in the plain 2780, and thence to *f* probably from 500 to 2000 feet.

Fig. 11.

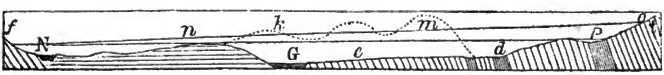

Figure 11 is a section across the Great Valley and the Swiss Plain from *t'* to *f*. The letters correspond (except P, incorrectly put for *p*); the horizontal scale is double that of the map, and the elevations here are in *French* feet above the sea.

t', The Great St Bernard—height 9000 feet above the sea.

At *o*, glacial traces exist at a height of 7794 feet.

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P, Mountain Plan-y-Beuf—where traces exist at 5445 feet.
d, Martigny—height 1480 feet above the sea.
e, The bottom of the valley at Monthey.
G, Lake of Geneva, 1150 feet above the sea.
k, Hill where Chalet of Playau stands—traces at 3760 feet.
N, Lake of Neuchatel 1340 feet.
f, Mount Jura—traces at 3444 feet.

The dotted outline from *m* to *k* indicates the position of the mountains forming the eastern boundary of the valley *d e*; *m* is the Dent de Morcles, 8940 French feet in height; *n*, the hills north-east from Lausanne.

The parallel lines in the figure indicate the position of the rock formations. At *f* on Mount Jura the limestone dips south-east at a pretty high angle. From N to G the Molasse, a sandstone, varies much, but has generally a slight dip to the south-east; from G to *t* the rocks consist of limestones and slates of different ages from the chalk to the Palæozoic series, with masses of granite or gneiss (marked by closer lines) intercalated at P and *d*. The stratified rocks here are highly inclined, and sometimes vertical. The figure is intended merely to convey a general idea of the form of the surface over which the glacier glided, and the lines *o f*, P *f*, do not indicate the true inclination of its surface, but one very much greater. The line *o f* dips at an angle of 5°, while the true dip of a line passing from the one position to the other is only 40′.

Slope of the Ancient Glacier.

Would the inclination before mentioned of 22′ or one foot in 156, *suffice to generate progressive motion in a glacier?*—Positive data for the solution of this question do not exist, but there are facts from which inferences may be drawn by analogy. To any one who knows nothing more of glaciers than what the eye tells, it may seem strange to say that these masses of ice are *plastic*, and have a motion like that of a semi-fluid body, such as tar or wet mortar. The lower end of a glacier is generally *a precipice of ice*, ten, twenty, or thirty feet high; and, in some cases, where it emerges from the valley, and projects into the plain, it has the form of a mound, very steep, both on the sides and front. On the upper surface are seen fissures two or three yards wide, and fifty or a hundred long, with vertical sides, and the lower end often presents galleries many yards in length, with upright walls, and large enough to permit a man to walk in them. To admit the *plasticity* of a body of this description seems somewhat like renouncing the testimony of our senses. That such, however, is really the constitution of glacier ice, has been proved in Professor Forbes's able and well-known "Travels in the Alps," to which the reader is referred for ample details. He describes glacier ice as traversed by an infinity of capillary fissures, and forming, in fact, a "congeries of tightly-wedged polyhedrons,"

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of the most irregular figures, and often three inches or more in length. According to Agassiz, it consists of fragments from half an inch to an inch and a half in breadth, increasing to three inches at the lower end of the glacier. The fissures, says Professor Forbes, admit the free infiltration of water to great depths, and impart to the mass "a certain rude flexibility within narrow limits." As evidence of this flexibility, it may be sufficient to mention two facts. First, the middle of a glacier moves faster than the sides, shewing that the constituent polyhedrons of the ice separate from, and glide over, or pass one another. Secondly, a glacier accommodates itself to the dimensions of its bed; it contracts its breadth when it has to pass through a narrow gorge, and expands again when the bed widens. Its motions, in short, resemble those of tar or mortar or mud on an inclined plane.

The extreme facility with which water obeys the law of gravitation is well known. The "rapid" Rhone, according to M. de Beaumont, has a mean fall of only 1′ 54″ or about 3 feet per mile, from Lyons to Arles. Nay, there are portions of the Rhine and the Seine, he says, where the declivity is so small as 8, or even 4 seconds—that is, a fall of one foot in 25,000 or 50,000. Sir Charles Lyell states that the surface of the Mississippi, at its junction with the Ohio, has an elevation of no more than 200 feet above the Gulf of Mexico. The round number of 200 may raise a doubt whether it is the result of careful measurement; but if so, the fall must be only about 2 inches per mile; for the length of the river below the junction, including all its sinuosities, is estimated in "Darby's" *Louisiana* at 1175 miles. The component parts of a glacier, however, want the mobility of the molecules of water, and the motion of the former is better illustrated by the troughs filled with plaster of Paris in Professor Forbes's ingenious experiment. We have another illustration in the flowing of a lava current, which, like the glacier itself, has the advantage of being on a grand scale. M. de Beaumont, in his valuable Memoir on Mount Etna, gives a table of the declivities or slopes of a great number of currents in active or extinct volcanoes. In 22 of these the slope was under 3 degrees; in 10 it was under 2 degrees; in 5 under 1 degree (that is, less than 1 foot in 57), and in the vast currents which flowed from the Icelandic volcano of Skapta Jokul in the terrible eruption of 1783, to an extent of 50 miles, the slope was only 30 minutes, or a fall of 1 foot in 114. This was the *mean* slope, and at some parts the actual fall must have been still less. Now, the fluidity of lava is much less perfect than that of water, even at the moment when it issues from the crater, and when passing through the pasty condition before it becomes solid, as in the lower part of a coulée, may fitly bear a comparison with glacier ice. In its most liquid state, a large stone thrown upon it floats on its surface, as a loaf floats on *honey*, which it resembles in consistency. A coulée, eight feet broad, which I saw

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on the top of Vesuvius, flowed sluggishly, according to my estimate, at the rate of a foot in five or six seconds, and had morsels of solid lava floating on it. In the eruption of 1631, Mr Auldjo found the lava the day after its eruption advancing in the low ground at the slow rate of *ten feet per hour*. The coulées of Etna are on a grander scale. Mr Scrope saw one "slowly progressing at the rate of about *a yard per day*," nine months after it had issued from the flank of the mountain; and other currents are described by Ferrara and Dolomein "as still moving on *ten years* after their emission,"—clear evidence of a pasty condition and very slow motion like that of a glacier. The pasty condition which lava assumes is further exemplified in the stringy forms and strange shapes into which it is drawn out or twisted, resembling coils of rope, horns, festoons, &c., and still better, perhaps, in the multitude of cells it contains, curiously elongated in the direction of its motion. Mr Scrope applies to it the terms "viscous, glutinous, ductile, semi-solid."—(*Considerations on Volanoes*, p. 102.) Again, there is a similarity even in the external form of the glacier and the lava coulée. The latter moves on between two ridges of scoriæ, or solidified portions of its own substance, as a glacier advances between lines of fragments torn from the rocks it has been in contact with. Both are resisted by friction on the sides and bottom of their channels; in both, owing to this resistance, the middle moves faster than the sides and bottom, and the upper surface is raised into a convex form. Further, the parallel flutings (*cannelures*) noticed on the surface by M. de Beaumont, are the counterparts of Forbes's "blue bands," and like them arise from the different parts of the current or coulée, moving with different velocities. In short, widely unlike as the substances are, there is no doubt that gravitation acts upon them very nearly in the same manner, and that if a *mean* slope of 30′, or one foot in 114, suffices to carry a *deep* coulée of lava over a line of 50 miles, there is a reasonable presumption that, with a declivity equally small, a glacier 2500 feet deep might advance from Martigny to Chasseron.

Professor Forbes, an excellent authority on such questions, considers it certain that the law which regulates the motion of the more perfect fluids, such as water, is applicable to the more imperfect, such as glacier ice (*Travels in the Alps*, p. 385, first Ed.) The effect of that law, in reference to the dimensions of a stream, is thus concisely enunciated—"A stream of twice the length, breadth, and depth of another, will flow on a declivity half as great, and one of ten times the dimensions upon one-tenth of the slope." Now, the mean slope of the Glacier de Bois over a space of three miles where it was most level, was found by the same author to be $4\frac{1}{2}$ degrees, or about 1 foot in 13; its depth, near the upper limit of that space, was reported to be 350 feet; but this was believed to be the extreme depth, and the mean for the three miles may perhaps be taken at 250 feet. Its rate of motion varies from day to day—small in winter, greatest in

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warm and wet weather. At a distance of 100 yards from the side, it was found to be 483 feet per annum, and in the middle it was estimated at two-fifths more, or 676. Now, if a slope of 4½ degrees gives motion to a glacier 250 feet deep, it follows from the rule laid down, that a slope only one-tenth of this—viz., of 27 minutes (or 1 foot in 127), would suffice for a glacier ten times as deep (and wide in proportion), such as the one which has left traces of its existence 3100 feet above Martigny, and 2780 feet above the Lake of Geneva at the Chalet of Playau. The question put was, *whether a slope of 22 minutes would be adequate?* and the result obtained (27′), is sufficiently near to shew that there is little force in the objection drawn from the small difference of level between the point P, whence the blocks come, and the point *f*, to which they were carried. It must be kept in mind, too, that the relative heights of *P* and *f* have not yet been determined geometrically. Professor Forbes spoke from careful consideration when he said—"We cannot admit it to be any sufficient argument against the extension of ancient glaciers to the Jura, that they must have moved with a superficial slope of one degree, or in some parts even of a half or a quarter of that amount, whilst in existing glaciers the slope is seldom or never under three degrees."

There is no other large glacier whose mechanical constitution and motions have been studied with the same care as those of the Glacier de Bois, but some interesting facts are given by Agassiz and Desor respecting the great glacier of the Lower Aar. In a length of 7000 metres from the Hotel Neuchatelois (a cave) to the top of the terminal declivity, the surface falls 486⅓ metres (Desor, "*Excursions*," p. 242) indicating an angle of 3° 58′, or a slope of one foot in 14¼ nearly. According to M. Martins, the lower part of the glacier in 1842 advanced 128 English feet per annum, the middle 233 feet, and the upper part 246. The more rapid motion of the Glacier de Bois is, no doubt, chiefly the consequence of its greater inclination, especially at the lower end. Of the depth of the glacier of the Lower Aar, we have no very satisfactory account; for Agassiz was unable to bore to a greater depth than 150 feet, and the depth obtained by sounding through a natural opening ("*moulin*") cannot be entirely depended on. The vertical height above the bed of the stream at the lower end (from *d* to *b*, fig 8) was found by Agassiz to be 96 metres, or 315 English feet.

It has been shewn that various phenomena connected with the transportation and deposition of boulders are better explained by the glacier, than by the iceberg, hypothesis. The latter seems equally deficient when applied to the case of striated and grooved rocks. Granting that in certain circumstances floating ice might produce *horizontal* striæ or groovings on even surfaces, how shall we account for the very common case of those groovings which incline downward, following the slope of the valley; or those occasionally seen which point upward; or those traced on the curved

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surfaces of hollow recesses, into which a large iceberg or floe could not enter? The glacier, on the other hand, striates rocks under these various circumstances before our eyes. A more radical difficulty yet applies to the iceberg theory. Whence did the water come on which the ice floated? Shall we say, from the sea? This would amount to the very bold assumption that the Alps had been submerged to the depth of 6000 or 7000 feet for many thousand years, and then raised up again, within the post-tertiary period! Passing over other objections, the assumption is refuted by the fact, that the Erratic Formation, or "Terrain Morainique" of the Swiss plain, rests, not upon marine beds, such as the sea in its supposed long sojourn should have left there, but upon a stratified deposit called the "Alpine Diluvium," which in its upper part contains the bones of the existing Swiss Mammalia, and at the bottom those of Elephas primigenius associated with fresh-water shells. Pictet, the learned palæontologist of Geneva, holds that the formation of this Diluvium belongs to the *modern* or current period, that its fauna was essentially the same with the present one, no new species having been added, but merely a few having died out. He thinks, however, that the Diluvium of Switzerland is more recent than the deposits bearing that name in Europe generally. (Pictet, Memoire sur des ossements trouvés dans les graviers stratifiés des environs de Mategnin. Geneve, 1845; C. Martins et B. Castaldi, sur les Terrains Superficiels de la Vallée du Po comparés a ceux du Bassin Helvetique, 1850). Perhaps it may be said that the icebergs floated on a natural lake. But if so, we ask, with Forbes, what were its boundaries, and where were the barriers which maintained a vast sheet of water at a level of 2000 feet above the surface of the country? "Such barriers cannot be pointed out, consistently with what is known as to the unchanged condition of the superficial deposit in Switzerland generally, since the period of the transport of erratics." On a survey, then, of all the facts known respecting the distribution of the Swiss boulders and the constitution and agency of glaciers, the evidence seems decidedly to preponderate in favour of Charpentier's doctrine, that the Alpine blocks found on Jura and in the plain were transported by glaciers. There are no doubt some difficulties attending this conclusion, but these may be removed by future research.

Ancient Glaciers' Rate of Motion.

Several questions present themselves respecting the glacial period in the Alps, to which satisfactory answers cannot be given. We cannot tell what caused it, or how long it endured, or what length of time has elapsed since it ceased—that is, since the glaciers retreated from the plain to those higher valleys in the mountains which they now occupy. Agassiz thinks that a fall of mean temperature equal to 8 degrees centigrade, or 14½ of Fahrenheit, would give the glaciers

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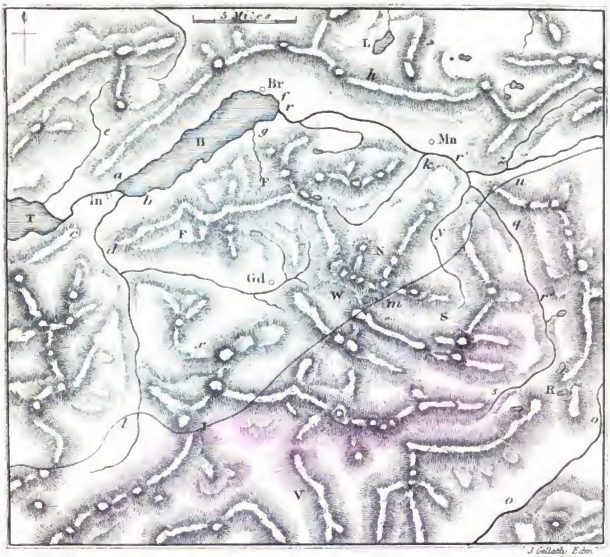
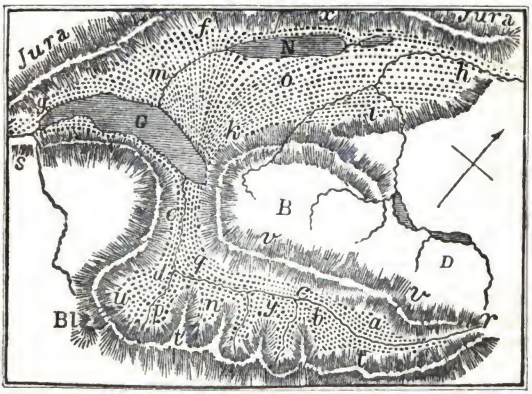
MAP I.—BERNESE OBERLAND.



MAP II.—WESTERN SWITZERLAND



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the requisite extension to carry the Alpine blocks to Jura, but the conclusion rests on hypothetical grounds. Charpentier holds that no unexampled intensity of cold is necessary, that 700 or 800 cold and wet years like those from 1812 to 1818, would be sufficient—an opinion in which probably few will join him. To determine the duration of the glacial period, it would be necessary to know the number of transported blocks, the distance which each travelled, and the velocity with which it moved—elements all beyond our reach. We are, however, able to say, that many of the blocks have travelled a hundred miles or more, and we may form a rude idea of the rate of their motion from data furnished by Forbes and Agassiz. The velocity depends mainly on the slope of the glacier, and its magnitude (or the area of its section), a greater slope compensating for a smaller sectional area, and a greater area for a smaller slope. On the most level part of the Glacier de Bois the motion at the middle was computed to be about 676 feet per annum. On the Glacier of the Lower Aar, Agassiz found that his tent travelled 64 metres (210 feet) in one year, and a comparison of older with recent observations shewed that a block had travelled 8 kilometres in 133 years, or 197 feet per annum (Martin's "*Revue de deux Mondes*," Mars 1847.) Anything like precision is unattainable, but 500 feet may be taken hypothetically as a rough average of the annual motion of the middle part of the ice (for the sides move much more slowly) in these two great glaciers. Now, in a great glacier extending from the Pennine Alps to Jura (*P* to *f*, fig. 10), the slope would be ten times less, and would go to diminish the rate of motion in some such proportion; but the sectional area, or measure of size, being ten times greater, would have precisely an opposite effect. Assuming, therefore, that the one would counterbalance the other, let us suppose that the blocks travelled on the great ancient glacier at the rate of 500 feet per annum. Then, by measuring the distances on Keller's map, we find that a block carried from the east shoulder of Mont Blanc to Chasseron (*u* or *p* to *f* in the map) would spend 740 years on its journey; one from the valley of Saas or Nicholas (*y*) to the same spot would spend 1000 years; and the huge boulder which made the "grand tour" from *t* to Steinhoff, near *h*, must have been 1600 years upon its travels! It is true, the medial and lateral moraines do not consist of isolated blocks, but of rows or trains of blocks, generally pretty close behind one another. The first boulder dropped on Jura would therefore have many others near and behind it, but still the depositation would be slow, for at the speed supposed, only as many as could lie on a length of 500 feet would be deposited in a year, and these spread over a considerable surface. We must remember, too, that the blocks from the Alps are scattered not only over the long line of Jura, but over all the western Swiss Plain, an area of nearly 3000 square miles. Many thousands of years would evidently be required

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for the threefold operation—of detaching the fragments small and great from the parent rock by the slow action of the elements—enabling them to perform such long journeys—and redistributing them over so wide a space.

Traces have been found of the former existence of glaciers in Mount Jura, in the Vosges, in Wales, Scotland, Norway, and Sweden; and there is no reason to doubt that the *glacial period* in all these countries was coincident with that of the great ancient extension of glaciers in Switzerland. The aspect during that long period, of all Europe northward of the Alps, or perhaps the Pyrenees, must have resembled that of Sweden in the gloomy months of winter. The genial powers of nature lay benumbed under a perpetual winding-sheet of snow and ice, covering mountain and valley, spreading death and hopeless sterility over the whole north, and the plains of Britain, France, and Germany, where flocks and herds now pasture, and rich harvests bloom, and mighty cities teem with millions of industrious men living in security and comfort.

The question remains, "What length of time separates the glacial period from that in which we live?" To this question no definite answer can be given, but it may be safely said that the glacial period had passed away long before the appearance of Man upon the earth.

Infusoria, the earliest Larval state of Intestinal Worms, according to Professor AGASSIZ.

Although for want of time, says Agassiz, in a letter to Mr Dana, my investigations on intestinal worms have been limited, I have arrived at one important result. You may remember a paper I read at the meeting at Cambridge (America), in August 1849, in which I shewed that the embryo which is hatched from the egg of a *Planaria*, is a genuine polygastric animalcule of the genius *Paramecium*, as now characterised by Ehrenberg. In Steenstrup's work on alternate generation, you find that in the extraordinary succession of alternate generations ending with the production of *Cerceria*, and its metamorphosis into *Distoma*, a link was wanting,—the knowledge of the young hatched from the egg of Distoma. The deficiency I can now fill. It is another infusorium, a genuine *Opalina*. With such facts before us, there is no longer any doubt left respecting the character of all these Polygastrica; they are the earliest

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larval condition of worms. And since I have ascertained that the Vorticellæ are true Bryozoa, and botanists claim the Anentera as Algæ, there is not a single type of these microscopic beings left, which hereafter can be considered as a class by itself in the animal kingdom. Under whatever name and whatever circumscription, it has appeared or may be retained to this day, the *Class of Infusoria is now entirely dissolved*, and of Ehrenberg's remarkable investigations, the descriptive details alone can be available in future; the whole systematic arrangement is gone.

This result has another interesting bearing; for it shews the correctness of Blanchard's view respecting the *Planariæ*, their close relation to the intestinal worms under the name *of Trematoda.* Indeed, they belong to one and the same natural group.

Is it not remarkable that the two types of the animal kingdom long considered as the fundamental supporters of the *theory of spontaneous generation* should have finally been brought into so close connection; and that one of them—the Infusoria—should in the end turn out to be the earliest larval condition of the other,—the intestinal worms being the parents of the Infusoria.*

On the General Distribution of Iodine. By Mr Stevenson Macadam, Teacher of Chemistry, Philosophical Institution, Edinburgh. Communicated by the Author.

The present investigation owes its origin to some observations lately made by M. Chatin of Paris, and communicated by him to the French Academy of Sciences.

Chatin is of opinion that in the atmosphere, in rain water, and in soils, there is an appreciable amount of iodine; that the quantity of this element present in one district, differs from that in another; and that the relative amount of iodine in any one locality, determines to a great extent, the presence or absence of certain diseases. For instance, in the district of country which he classifies under the general title of the *Paris zone*, the quantity of iodine present in

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* *Vide* Silliman's Journal, May 1852.

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the atmosphere, in the rain water, and in the soil, is comparatively great, and to this he ascribes the absence of goitre and cretinism; whereas in the zone corresponding to that of the *Alpine valleys*, the amount of iodine has diminished to one-tenth of that found in the *Paris zone*, and to this scarcity of the element, he attributes the prevalence of goitre and cretinism, which in that zone are endemic.* Considering that the subject was one of great importance, more especially if the conclusions arrived at by Chatin—in reference to the functions fulfilled by iodine, in preventing the occurrence of the diseases referred to—could be legitimately deduced from the experiments which he performed, I have recently undertaken a series of analyses in reference to the general distribution of the element in question.

My attention was principally directed to the atmosphere, and to rain water, both of which, apart from the observations of Chatin, I had reason to believe would contain iodine. It is well known that consequent on the evaporation of water from the surface of the ocean, portions of the salts contained in it are carried up, and disseminated through the atmosphere, ready to be rained down upon inland places, and that from this source iodine—principally as iodide of sodium—will most probably reach the air. This constant supply will be further augmented by the iodine vapour which is disengaged from many mineral springs, and which, amongst other possible compounds, will more especially exist in the atmosphere as iodide of ammonium. Independently, therefore, of any experiment, I thought it in the highest degree probable, that iodine would be found present in the conditions referred to, viz., as iodide of sodium and iodide of ammonium, and it only remained to determine whether or not the *quantity* of iodine was so great as to come within the range of our most delicate tests.

I commenced with the atmosphere. The process followed was identical in principle with that pursued by Chatin. From statements made in different parts of one of his memoirs, it would appear that the apparatus he employed was a series of Liebig's bulbs containing a solution of carbonate of potassa, and attached to an aspirator by means of which air was drawn through the liquid.† In the arrangement I employed, the air was made to traverse,—

1. A wide tube, containing slips of paper moistened with solution of starch; and,

2. A double-necked gas bottle, containing three ounces of a dilute solution of caustic alkali.

At the commencement of the experiment, caustic soda was placed in the bottle (2) and not less than 150 cubic feet of air drawn through. The soda was then replaced by caustic potassa, and a similar volume

* Comptes Rendus, tome xxxiv., p. 51; and Edin. New Phil. Journal, No. 104.
† Comptes Rendus, tome xxxii., p. 669.

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of air passed through it. At the conclusion, the papers, over which 300 cubic feet of air had been drawn, were carefully inspected, but not the slightest indication of iodide of starch could be detected, even when moistened with distilled water. The soda and potassa were separately treated with starch and nitric acid, and both exhibited the rose-colour characteristic of the presence of iodine in small quantity. At this stage of the inquiry, I entertained great hopes of being able to verify Chatin's observations; but, on analysing portions of the original alkaline solutions through which no air had been drawn, I found iodine present in them in quantity, to all appearance, as great as it was in those portions of the liquids used in my experiments.

The caustic alkalies employed by me, were therefore contaminated with the very substance I was searching for in the atmosphere, and it remained to inquire into the original source of this impurity. With this view, I tested samples of the carbonate of potassa, carbonate of soda, and lime-shell, which had been employed in the preparation of the caustic solutions, and in all three, iodine was present in perceptible quantity. Desirous of making certain that the agents used by me were as pure as other commercial substances of the same kind, various specimens of each were obtained and submitted to the process to be afterwards detailed. The samples first tested were those usually to be purchased in Edinburgh and other places, but subsequently genuine and authenticated specimens were procured from trustworthy sources; and from every sample of carbonate of potassa, carbonate of soda, and lime-shell, which I have as yet subjected to examination, I have obtained distinct indications of the presence of iodine. It became, therefore, quite evident, that so far as the determination of iodine in the atmosphere was concerned, the experiments as yet referred to, were of no value, and that it was requisite, in any future experiment upon this subject, to avoid the introduction of the alkalies, which so invariably contained this element as a foreign ingredient.

Accordingly, in the next experiment the alkalies were dispensed with, and their place was occupied by nitrate of silver. The apparatus also was somewhat modified—the air being drawn through—

1. A tube with slips of starched paper, kept somewhat damp.
2. A gas bottle immersed in a freezing mixture; and,
3. A gas bottle containing a solution of nitrate of silver.

To enable the condenser (2) to do its work thoroughly, and to guard against any of the liquid in the gas bottle (3) being carried away by excessive evaporation, they were buried in soil which was saturated with water. A continuous current of air was kept up for fully five hours, commencing at mid-day. At the conclusion of this experiment the papers were not altered in the slightest degree; the gas bottle (2) contained about a quarter of an ounce of liquid; and the nitrate of silver (3) had not been perceptibly changed. The condensed liquid was neutral to test-papers—a drop of starch was

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added to it, and subsequently hydrochloric acid and nitrite of potassa, which together form a most delicate means of detecting iodine*—the result was negative. The nitrate of silver solution was cautiously evaporated to nearly a quarter of an ounce, a stream of sulphuretted hydrogen passed through to precipitate the silver, and liberate as hydriodic acid any iodine which might be present—the liquid raised in temperature, carefully avoiding ebullition, and filtered. The filtrate, on the addition of starch, hydrochloric acid, and nitrite of potassa, did not exhibit the slightest trace of iodine. I therefore concluded, that in the large volume of air—upwards of 300 cubic feet—which had been drawn through the arrangement, there had not been an appreciable amount of iodine, either in a free state (in which case the starched paper would have been acted upon), or combined with a metal or base (in which condition it would have been detained by the nitrate of silver, forming iodide of silver).

The experiments referred to, were made at different heights on Arthur Seat, and their negative results led to arrangements being made for a trial on a larger scale. Through the kind permission of the proprietor of Kinneil Iron-Works, I was enabled to proceed to Borrowstonness and attach my apparatus to the receiver from which the air under great pressure is forced into the blast-furnaces. By means of a stopcock fixed in the receiver and a long flexible tube, the air was conducted to the following arrangement:—

1. A wide tube, containing slips of paper dipped in starch.
2. A condensing worm, nine feet in length, surrounded by a freezing mixture, and attached to a receiver.
3. A tall jar, containing chips of pumice-stone, and a few iron filings, with sufficient water to cover them.
4. A similar jar, with pumice-stone, scrapings of clean lead, and a solution of acetate of lead.
5. A condensing worm, nine feet in length, immersed in a freezing mixture, and attached to a receiver.

By this arrangement it was expected that the first condenser (2) would retain the water, vapour, and salts, which the air experimented upon held in suspension, and should the accumulated liquid be sufficient to fill the tube, the excess would be projected into the receiver, and thus be kept from passing into other parts of the apparatus. The jar (3) was capable of retaining any free iodine, and was intended as an auxiliary to the papers (1). The chips of pumice-stone enabled the air, as it gurgled through the several layers, to come in contact with the reagents contained in the jar. The office to be fulfilled by the solution of lead in jar (4) in retaining

* Quarterly Journal of the Chemical Society, vol. iv., p. 155. Dr Price says, "In this manner I have detected the $\frac{1}{4,000,000}$th part of iodine dissolved in water, as iodide of potassium."

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any compound of iodine, will be at once apparent. The condenser (5) was intended to liquefy any watery vapour which might, during the experiment, be carried from the jars (3) and (4).

The air, under a pressure of 3 lb. on the square inch, was allowed to traverse the arrangement for fully four hours, during which time upwards of 4000 cubic feet had been brought in contact with the reagents employed. The apparatus was then taken asunder, and the contents of the vessels being placed in stoppered bottles, the whole was brought to Edinburgh for examination. The slips of paper (1) were not sensibly altered in tint, and did not betray the slightest indication of even a rose colour when moistened with distilled water. The condensers (2 and 5) contained each a very small quantity of liquid, which, on being tested, did not shew a trace of iodine. The contents of the jar (3) were thrown on a filter and washed with cold water. To the filtrate was added some drops of a solution of carbonate of potassa, and the liquid thus rendered alkaline was evaporated to a quarter of an ounce; no iodine was present. The carbonate of potassa used in this trial was prepared by calcining cream of tartar at a white heat, and was so far free from iodine, that none could be detected in two ounces of a dilute solution, of which, in testing the contents of the jar, I employed less than half an ounce. There was, therefore, no likelihood of a perceptible quantity of iodine being added in the minute portion of alkali used, even though the analysis of the contents of the jar had shewn its presence. The jar (4) with the lead solution was treated in the same manner as described in a former part of this paper, when referring to the employment of silver, and the result was also negative.

Notwithstanding the large scale on which this experiment was conducted, I still felt disinclined to pronounce a decided opinion on the subject, and resolved to make another trial on a much larger scale than either of those yet referred to. Accordingly I fitted up an apparatus of a larger size and more durable nature, which was carried to Kinneil, and attached, as before, to the condensed air receiver. At this time the air was passed through—

1. A capacious double-necked gas bottle, about two-thirds filled with distilled water.

2. A wide tube, containing starched papers.

3. A capacious gas bottle, containing pumice-stone, distilled water, iron filings, and a little sugar.

4. A similar bottle with pumice-stone, and solution of acetate of lead.

The object in passing the air through the distilled water was to load it with aqueous vapour, so that it should have less influence in causing evaporation of the liquids in 3 and 4. The sugar added to the bottle (3) was intended to prevent the oxidation of any protiodide of iron which might be formed, and which decomposition

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would have risked the loss of iodine.* The other parts of the arrangement need no comment.

For six days the air unceasingly traversed the arrangement, and at the conclusion not less than 100,000 cubic feet of elastic fluid had passed through. I was unable to watch the experiment throughout the entire period of its continuance, so that, after securely arranging the apparatus, and witnessing the commencement of its action, I confided it to the charge of Mr John Begg, the intelligent manager of the iron-works, who kindly took care of it till the close of the period mentioned. I then dismantled the arrangement, and transferred it to Edinburgh, where the results of the experiment were ascertained. The gas bottle (1) was destitute of liquid. At the lower part and around the sides saline matter in small quantity was attached. On rinsing with distilled water, this was easily washed out, and starch, hydrochloric acid, and nitrite of potassa, were added to it. No iodine was present. The starched papers (2) were not sensibly altered in tint. The contents of the bottles (3 and 4) were severally tested, as in the previous experiment, and no iodine was present.

From these results it was apparent, that in the large volume of air subject to examination, there had not been an appreciable quantity of iodine. Theoretically there is every probability of iodine and bromine being present in the atmosphere; the latter in much greater quantity than the former; and it is only after such repeated failures that I have come to the conclusion that the quantity of iodine in the atmosphere is frequently too minute for detection by the ordinary methods of testing.

The weather during each of the experiments was favourable to the object I had in view. Several sunny days preceded each of the trials, and in general the wind was north or north-east; in other words, blowing from the Frith of Forth landwards. The volume of air experimented upon was in every case larger than that used by Chatin. So far as I can gather from his papers, he employed 4000 litres of air at Paris, which contained $\frac{1}{5000}$th of a milligramme of iodine.† This is equivalent to 880 gallons of air producing $\frac{1}{3000.5000}$th of a grain of iodine, or $\frac{1}{2500.5000}$th of a grain of iodide of sodium. The volume of air employed in the first unexceptionable experiment which I made, viz., that where nitrate of silver was used, was 300 cubic feet, or about 1870 gallons, which, calculating from Chatin's observations, and considering the delicacy of the test I used, ought to have given satisfactory indications of iodine. In the first of the Kinneil experiments, 4000 cubic feet, or 25,000 gallons of air passed through the arrangement, and this, according to the same standard, ought to have given very distinct proof of the presence of

* Gmelin's Handbook of Chemistry, Watt's Translation, vol. v., p. 248.

† Comptes Rendus, tome xxxii., p. 669.

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iodine. In my last experiment, where I subjected to examination 100,000 cubic feet, or 625,000 gallons, I ought at the same rate to have obtained several ounces of a liquid, every drop of which should have attested the presence of iodine.

At the intervals which elapsed between the trial of these experiments, I was examining large portions of the rain water which fell in Edinburgh during this summer. I was careful not to employ the alkalies in any shape, although I was led to infer from Chatin's papers that potassa had been used by him.* In the first experiment I added to three gallons of the water some ounces of a solution of acetate of lead. On standing twenty-four hours, a precipitate had fallen to the bottom, from which the liquid was drawn off. The precipitate was treated, as described in a former part of this paper, and no iodine was detected. As the iodide of lead is slightly soluble in water, and might have been present in the liquid which had been removed from the precipitate, this liquid was evaporated to one ounce, and afterwards tested for iodine, but none was found. A second experiment was tried with a similar volume of rain water, viz., three gallons, substituting nitrate of silver for acetate of lead; a precipitate was observed after standing twenty-four hours, but neither it nor the liquid contained a trace of iodine. To a third quantity of twelve gallons, I added acetate of lead, and without separating the precipitate from the liquid, the whole was evaporated down to one ounce, and tested for iodine, but without giving a positive result. Another experiment made with three gallons of rain water, which had been collected at Unst, in the Shetlands, and to which acetate of lead was added, gave also a negative result.

The proximity of Edinburgh to the sea, the direction of the prevailing winds, and the falling of the rain used in these researches after somewhat lengthy droughts, all tended to make the rain water of the district in a condition highly favourable for the object in view. This remark applies with special force to the water received from Unst which had fallen in the immediate vicinity of the ocean. Chatin announces that the proportion of iodine in rain water is very variable. At one time 10 litres of water collected at Paris, gave one-fifth of a milligramme, and at another time, the same quantity of water produced half a milligramme. Did all rain water contain as much as the least of these quantities, then a very distinct coloration would be exhibited by one gallon evaporated to a quarter of an ounce. In my researches just detailed, three and even twelve gallons were found insufficient to give the faintest indication.

So far, then, as my investigations on the presence of iodine in the atmosphere and in rain water are concerned, I am forced to believe that at the time I experimented, there was not a sufficient quantity of this element present in either, to respond to the delicate test I em-

* Comptes Rendus, tome xxxi., p. 282.

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ployed. At the same time I admit the possibility, that at other seasons of the year, and at other districts of the country than those in which I experimented, there may be an appreciable amount of iodine naturally distributed in the way referred to, and when time and opportunity present themselves, I shall not fail to continue my investigations on this department of such an important subject.

The locality being unknown to me from which the lime-shell employed in the preparation of the caustic alkalies had been procured, I afterwards obtained specimens from Burdiehouse, Kirkcaldy, Charleston, and Bathgate, and examined them according to the following process. To a portion of each was added about a gallon of distilled water, which was well agitated at intervals till it was completely saturated with lime. The liquids thus obtained were nearly neutralised with pure nitric acid, and separately evaporated to one half ounce. On being treated with starch, hydrochloric acid, and nitrite of potassa, distinct evidence was obtained of the presence of iodine in each of the four specimens.

In a former part of this paper, reference was made to several samples of potashes in which iodine had been discovered experimentally. Altogether, I tested six different specimens; two of crude potashes, one of them from the United States, and the other from Canada; two of refined, or what is ordinarily termed carbonate of potassa, and two of bicarbonate of potassa. They were all examined in the same way. A large quantity of the salt was drenched with distilled water, cautiously raised in temperature and allowed to cool. This served to separate the larger portion of the carbonate of potassa, as well as any impurities present in the crude samples. The liquid containing the iodide of potassium, was transferred to another vessel and evaporated to dryness. The resulting salt was then powdered, alcohol added, raised in temperature, and filtered. The filtrate was again brought to dryness, the residue digested in a very small quantity of water, and the solution thus obtained treated with starch, hydrochloric acid, and nitrite of potassa. In each case, a very distinct coloration was obtained. The crude potashes contain in fact, a considerable quantity of iodine which decreases at each refinement.

Six samples of soda ash were examined in the same way. Like the potash specimens, two were of the crude soda ash, two of the ordinary carbonate of soda, and two were bicarbonate of soda. The crude variety contains the most iodine, and the others less of this impurity according to the refining they have undergone.

From the presence of this element in potashes, I am inclined to believe that it will be found more generally distributed in the vegetable kingdom than it has formerly been supposed to be. The potashes from the States and from Canada, are principally the dried lixivium of hard woods, such as the maple and birch; but whilst by much the greater portion is so, it is probable that the parties in

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charge, are not very scrupulous as to the plants they employ, and do not hesitate occasionally to add all vegetable matter which comes in the way. It may therefore be objected to the statement that forest trees contain iodine, that the iodine found in the pot-ashes may be derived from succulent herbs and shrubs, and not from the trees themselves; but this objection will be at once removed when it is stated, that in the lixivium of charcoal, I have found very distinct traces of iodine. The charcoal sold and used in this country, is principally oak, with a little beech, birch, elm, and ash; and after obtaining satisfactory evidence that the ashes of these woods burned indiscriminately, contained this ingredient, I burned large quantities of the first three kinds, viz., oak, beech, and birch, and treated the ashes in the same way as the potashes. Iodine was distinctly present in all three. The amount of iodine in forest trees must be comparatively small. When experimenting with potashes, one is apt to forget the small bulk into which a large quantity of timber falls, when the organic matter is expelled, and the saline ingredients are alone left. So far as can be estimated from the present qualitative experiments, the relative quantity of iodine in forest trees is much less than that in succulent plants growing in marshy places.

The constant presence of iodine in potashes will lead to some considerable alterations in the methods generally followed for the detection of the former, by a process which necessitates the use of the latter. The process for iodine in cod-liver oil, where potassa is added to saponify the oil:* that for iodine in sea-water where potassa is added to precipitate the alkaline earths:† that for iodine in coal where potassa is added to the ammoniacal liquor for the purpose of fixing this element as iodide of potassium:‡ and amongst others, that for iodine in soils where potassa is added for the purpose of more readily extracting the iodine from them,§—must all be modified.

For some time back I have also been engaged in collecting and testing a large number of plants growing in different places. Although it is now generally recognised, that iodine is a constituent of some fresh water, and even a few strictly land plants, yet still the volatilization of the iodine renders the success of such an investigation so uncertain, that the names of few plants have as yet been published, in which iodine has been detected. The difficulty lies principally in properly burning the plants to ashes. When iodide of potassium is heated strongly alone, it volatilises, whilst if accompanied by carbonaceous matter, carbonate of potassa is formed, and iodine vapour escapes. From experience, I feel certain that

* De Jongh on Cod-Liver Oil, translated by Dr Carey.

† Dr Schweitzer on the Analysis of Sea-Water as it exists in the English Channel, near Brighton; Lond. and Edin. Phil. Mag., vol. xv., p. 53.

‡ M. Bussy; Comptes Rendus, tome xxx., p. 538.

§ M. Chatin; Comptes Rendus, tome xxxiv., p. 52.

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a great many of the failures to find iodine are to be attributed to this—and that not only in the analyses of plants, but also in testing for iodine in cod and skate liver oils, where the practice has been to add caustic potassa and incinerate at a high temperature. In such cases, notwithstanding that the oil probably contained iodine, and that it was certainly present in the potassa, yet, after examination, it has not been detected in the ashes. To avoid the loss of iodine thus sustained, Chatin recommends the addition of *potassa* to the plant previous to incineration.* But this, whilst it will no doubt, to a certain extent, hinder the volatilisation of iodine, will not ensure its retention; and moreover, the saturation of the plant with ordinary potashes, necessarily causes the addition of the very element that the experimenter is in search of. The only safeguard which I have adopted is to burn the plant in a chamber with a small quantity of air, and where there is little draft. In this process it can hardly be said that the plants are *burned*—the term should rather be that they are *charred*. They are then finely powdered, digested in hot water, and filtered; the clear liquid is evaporated to dryness and subsequently treated like the potashes.

In the following list of plants there are representatives from different districts and from different altitudes. In the majority of cases a large quantity of the plant was used in the examination, and so far as could be inferred from the depth of the rose or blue tint assumed by starch, the quantity of iodine in different plants was very various. But as no attention was paid to the weight of the original bundles of dried plants, or even to that of their ashes, I would refrain from speculating as to any law which might regulate the increase or decrease of iodine in plants belonging to different natural orders, or grown in dissimilar situations. Moreover, there are a number of plants in which I have failed to detect iodine; and whilst it is probable that some or most of them may be destitute of that ingredient, yet, considering the many ways in which so volatile a substance could have escaped, I propose to make other trials with those *negative* plants, before I announce their names, and the localities from which I received the specimens worked upon.

As having some connection with the subject treated of, I would intimate that I have obtained distinct indications of the presence of bromine in crude potashes. It is unfortunate that our tests for bromine are so much inferior in delicacy to those for iodine, that it is necessary to operate upon very large quantities before the indications of the former element are distinct. There is no doubt that, from the presence of bromine in trees, it will be found in greater abundance in the more succulent plants; but the few trials I have yet made have been unsuccessful in determining its presence in any but the crude Canadian and American potashes.

* Comptes Rendus, tome xxx., p 354.

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Table of Plants in the Ashes of which Iodine is present.

(A.) *In the following Plants, hitherto unknown to contain Iodine, I have detected that element.*

Botanical Name.	Locality of Specimens Examined.
Ranunculus aquatilis.	Dunsappie Loch.
Stellaria uliginosa.	Langley well, one of the tributaries of the Leithen.
Alchemilla vulgaris.	Dry slopes at Grey Mare's Tail, Dumfriesshire.
Chrysosplenium oppositifolium.	Langley well, one of the tributaries of the Leithen.
Galium verum.	Cockburn's grave, St Mary's Loch.
Achillea Millefolium.	Do. do.
Senecio Jacobæa.	(1) Valley of the Leithen, and (2) Cockburn's grave, St Mary's Loch.
Centaurea nigra.	Valley of the Leithen.
Vaccinium Myrtillus.	Windlestraw Law.
„ Vitis-idæa.	Do.
Menyanthes trifoliata.	Duddingston Loch.
Myosotis palustris.	Do.
Digitalis purpurea.	Valley of the Leithen.
Veronica Beccabunga.	Ditches in the valley of the Leithen.
Mentha sativa.	Duddingston Loch.
Empetrum nigrum.	Windlestraw Law.
Betula alba *or* glutinosa.	Unknown.
Fagus sylvatica.	Do.
Quercus Robur.	Do.
Juncus conglomeratus.	Marshy places on banks of the Leithen.
„ lamprocarpus.	(1) Do. do. and (2) shores of St Mary's Loch.
„ squarrosus.	(1) Do. do. and (2) do.
Sparganium ramosum.	Quair stream, south side of the Tweed.
Potamogeton densus.	Dunsappie Loch.
Carex Œderi.	Quair stream, south side of the Tweed.
Equisetum arvense.	Cultivated places on the banks of the Leithen.
„ limosum.	Duddingston Loch.
Lastrea Filix-mas.	Elibank, south side of the Tweed.
Athyrium Filix-fœmina.	Do. do.
Asplenium Ruta-muraria.	Valley of the Yarrow Lochs.
Pteris aquilina.	(1) Valley of the Leithen, and (2) Valley of the Moffat waters.
Chara vulgaris.	Dunsappie Loch.
Sphagnum acutifolium.	Windlestraw Law.
Trichostomum lanuginosum.	Do.
Polytrichum commune.	Do.
Hypnum rutabulum.	Langley well, one of the tributaries of the Leithen.
„ triquetrum.	Elibank burn, south side of the Tweed.
Usnea plicata. Evernia prunastri.	Growing on trees of the natural order Coniferæ, in Glenormiston woods, Peeblesshire.

(B.) *I have confirmed the presence of Iodine in the following plants, in which it has been previously found by other observers. The plants are, however, from different localities.*

Nasturtium officinale.	(1) Marshy places on the banks of the Leithen, and (2) Duddingston Loch.
Iris Pseud-acorus.	Duddingston Loch.
Phragmites communis.	Do.

And in the ashes of Coal, representing the Flora of the Carboniferous era.

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The greater number of the experiments connected with this inquiry were conducted in the laboratory of Dr George Wilson, to whom I am deeply indebted for the kind manner in which he has afforded me every assistance in his power, during the whole course of the investigations.

Some Additional Observations on the Superficial Colouring Matter of Rocks. By JOHN DAVY, M.D., F.R.S.S. Lond. & Ed. Communicated by the Author.

In an excursion recently made into the wilds of Connemara, my attention was recalled to the superficial colouring matter of rocks, from certain marked contrasts of colour observable in adjoining rocks of the same quality, but differently situated. These contrasts presented themselves most conspicuously in the beds and banks of certain streams, and on the shores of certain lakes, especially at and near their margin.

Of the first mentioned, a good example occurs in the bed and banks of the small mountain-torrent which falls into Singalla Lake, immediately below Flynn's or Half-way-house (the designation on the map of the county Galway) on the road between the town of Galway and Clifden. There, in the bed of the stream, on the same rocks—a variety of mica slate—at least four distinct colours are noticeable. Of these one is almost white, in localities exposed to the full force of the stream when highest and of most force, or when swollen after heavy rains; a colour belonging to the rock in its worn and weathered state. Another is of a light red or reddish-brown hue, which appears on rocks in the middle of the little stream, such as are commonly under water, and where the water runs rapidly,—a hue owing, in this instance, to a slight deposition of peroxide of iron, constituting a superficial stain. A third is black and glistening, noticeable more partially, in spots here and there, towards the margin of the stream, and amongst the pebbles in its marginal rocky hollows,—a colour resulting also from a superficial stain, but produced chiefly by adhering peroxide of manganese. Another, the fourth colour, is also black, but with little or no lustre, occurring on the marginal rocks of the stream, subject to alternations of wet and dry, according to the state of

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the stream, whether high or low ; a colour, not like the two preceding, owing to a mineral stain, but to the growth and death of minute cryptogamic plants.*

On the shores of the lakes of this pre-eminently lake district, the differences in the superficial colouring of the rocks are chiefly two or three, and depending mainly on the cryptogamic vegetable covering. Black is the prevailing colour of the rocks, at the very margin of the lakes, whilst white, in many instances, is as conspicuously prevalent in the higher adjoining situations, out of the reach of water, when the lakes are at their greatest height. On the shores and islets of Derryclare Lake so distinguished for its beauty, and on those of Lough Inagh, a neighbouring lake, good examples are to be seen of rocks thus coloured; the white by a lichen, Lichen lacteus ; the black by one or more of the lower cryptogamia undergoing decomposition, and acquiring a peaty character, to which, in all of this tribe under the influence of moisture and a comparatively low temperature, there appears to be so great a disposition, as is indicated in the vast extent of bog for which Ireland generally, and Galway especially, is so remarkable.

In the examples mentioned, the instances of the several kinds of superficial colouring are well defined, occurring together. In other localities, occasionally only one colour is found predominant, as different shades of red where the rocks and gravel are stained by the peroxide of iron ; or of brown and black where they are stained both by the peroxide of iron and by the peroxide of manganese. Of the former a good example offers in the bed of the river descending through Glen Inagh into the lake of the same name ; and of the latter,

* In the bed of the same rivulet, nearer the lake, where its course is less rapid, an example occurs of a conglomerate rock in the act of formation, which may be worth mentioning,—the pebbles washed down, and there resting, finding as it were a matrix in the clay into which they are cemented by carbonate of lime. The induration of this conglomerate is not considerable ; but it is easy to imagine how it may be greatly increased, either by the deposition of more carbonate of lime, the proportion present being very small, only just sufficient to effervesce slightly with an acid, or by the action of heat, or other metamorphic agency.

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in the beds of many of the small streams emptying themselves into Lough Oured, and the Lake Singalla.

The methods by which I have ascertained the nature of the colouring matter, are of the simplest kind. I shall briefly mention them, as without such aids, merely by inspection, the quality of the matter imparting the adventitious colour could hardly be determined. The principal means I have employed have been an acid,—strong muriatic, the blow-pipe, and the microscope. Immersed in the acid, proof is afforded of the presence of black oxide of manganese by the solution of the colouring matter investing the pebble or fragments of rocks subjected to the trial, and by the evolution of chlorine; and of peroxide of iron by a slower solution of the colouring matter, without the disengagement of chlorine, but with the production of the odour belonging to the perchloride of iron in solution. Under the microscope, the structure of the vegetable matter is distinctly brought into view; whilst by the blow-pipe, the former is either destroyed, or if the apparent structure be retained, it is as a skeleton in the residual ash.

I have spoken of the vegetable colouring matter being owing to cryptogamia in a state of decomposition, or of transition into peat. This is probably true in most instances. In some, the black hue may be produced in a different manner, if not natural and belonging to the plant, viz., by the entanglement of peaty particles amongst the green leaflets and fibres of the plants. Instances of the kind I have seen distinctly when examining the vegetable matter with a low power, as with a one-inch object-glass; then, some portions of the plants have appeared of a healthy bright green, whilst others adjoining have been quite black. Indeed, in the majority of instances, as seen under the microscope, the appearance of the vegetable matter is not uniform, but more or less varied, a part only being black,—brown and greenish fibres being commonly intermixed; though, as seen with the naked eye, the whole appears black. I may add in confirmation that the line used in fishing in these lakes had acquired, even in a few days, a grey discolouration.

In a former note, I have supposed the black stain imparted

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by the peroxide of manganese to the rocks and pebbles in the beds of rivers on which it is found, to be owing to the precipitation of the oxide from its state of solution as a suboxide, on its becoming saturated with oxygen, and passing into that of the peroxide, after the analogous manner in which the stain by iron is produced in similar localities, and under similar circumstances. The further observations I have had an opportunity of making seem to corroborate this.

The superficial discolouration of rocks from the causes assigned is, I believe, of wide extent, and consequently not unimportant, considered merely in relation to the aspects of nature. The mineral stains—the ochry of iron, and the rich black of manganese, may be expected to be seen wherever water impregnated with carbonic acid gas,—as all rain water, the feeder of springs, more or less is,—percolates through, before appearing at the surface, strata containing these metals in the state of suboxide. And the dark vegetable stain, that resulting from the partial and peculiar decomposition essential to the formation of peat, may be looked for wherever the circumstances of average moisture and temperature of climate are favourable to the production of peat—a wide extent, comprising most parts of England, Ireland, and Scotland, and the greater portion of the north of Europe. These are not merely theoretical inferences; they are in accordance with many observations made both in the Lake District of England and in the Highlands of Scotland, and in the former much extended, as to localities, since I made the first communication on the subject, published in a former number of the *Philosophical Journal*. As regards the dark discolouration from decomposing vegetable matter, I may add that I have found it not only on rocks, on the shores of lakes and moors, where the circumstances have favoured, but also on the sea-shore and on inland precipices, where there has been a growth and decomposition of minute cryptogamic plants. A good example of the kind may be mentioned as occurring in the neighbourhood of Oban, at the entrance of Loch Etive, in Argyleshire. Specimens of rocks, so discoloured superficially as to be of a dead black, brought from thence, which I collected myself,

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some from the shore, within reach of the salt spray and occasionally washed by the waves, and some from a moist inland cliff not far from the sea, close to the town, examined in the manner described above, afforded similar results.*

LESKETH HOW, AMBLESIDE,
August 30, 1852.

On the Place of the Poles of the Atmosphere; and the Reid Theory of Hurricanes. By Professor C. PIAZZI SMYTH.

This is merely a notice on some of the recent discoveries and generalisations, by Lieutenant Maury, U.S.N., on the motions of the atmosphere. It had been clearly proved by the extensive researches of Lieutenant Maury, that the trade-winds when rising at the equator, do not, as previously held, return to their own poles, but cross over to the opposite ones; and thus traverse the extent of the whole earth from pole to pole, in a curvilinear direction, on account of the effect of the rotation of the earth. The whole atmosphere thus partakes of a general movement, the upper half moving towards the poles, and the lower towards the equator, or *vice versa*, according to the latitude of the place; the former occurring between the parallels of 0° and 30°, and the latter between 30° and 90°. At 0° and 30° two nodes, so to speak, of the upper and lower currents take place; at the former ascending, and indicated by a low barometer; at the latter descending, and marked by increased barometric pressure. At the point of 90°, the pole, or thereabouts, the revolution of the currents and their change of direction for N. and S., and *vice versa*, with another node, takes place, and marked, Lieut. Maury *thought*, by a *calm* region, as the two nodal zones of 0° and 30° most undoubtedly are.

As to the place of this calm polar point, which we shall probably long want observations to determine, Lieutenant Maury did not place it over the poles of rotation of the world, but over the magnetical poles, without, however, sufficient reason. Indeed, he much lamented that after the admirable developments made by Lieutenant Maury of the motions of the atmosphere, he should have thus brought in merely the *name* of magnetism to clear up one obscure point. Meteorology pursued on the system of strict mechanical and scientific inquiry was now disclosing a most interesting and understandable series of phenomena, and promised a legitimate har-

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* In some instances, the black hue which I have attributed above to the decomposition of the vegetable matter in transition to form peat, may be in great measure the natural colour of the species of cryptogamia covering the rock.

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vest of more. But the history of this science in times past, points to so many occasions when rational trains of observation were impeded by the gratuitous introduction of a magnetic or electric element, and thought to be needless thereafter, that the author supposed that it might be of some service to shew that there was no probability in the present case, either from actual observation or natural considerations, that such a force should be looked to for explanation.

1*st*, Of actual observation. The poles of any force should bear a certain known relation to the equator thereof; and if we find the magnetic equator coincident with that of the atmosphere, which may be considered as marked out by the line of equatorial calms, we might reasonably suppose a connection between their poles. But we do not. The mean positions of these equators are very different from each other, and are subject to such totally different movements through the year, that we cannot legitimately expect any nearer coincidence in their polar points.

2*d*, Of natural considerations. Mechanical force may always be taken as the cause, and not as the consequence, of the magnetic or electric currents by which it is accompanied. Certainly in the case of an electrical machine, the electric spark may be made to produce mechanical energy, as shewn in knocking small light pith balls about; but how incomparably less is this force to that employed to turn the machine round in the first instance to produce the electricity.

Now, the atmosphere enveloping and rubbing over the world, may be taken as a large electrical machine, and does produce electric and magnetic forces; but these, although startling enough when witnessed by us, little pigmies of men, are of infinitely small moment compared to the force required to keep the whole atmosphere in motion, and to overcome its friction and inertia.

Again, with regard to the intensity of terrestrial magnetism, it is found with one of Gauss's large bars for determining the horizontal force, by being suspended by two wires separated in the direction of its axis, that the whole magnetic force amounts to less than 100,000th part of the weight of the bar, that is, the force or attraction of gravity.

Similar experiments might be adduced, to shew that when a body is heated, though electrical currents may be produced, and may have a certain mechanical power, that yet the quantity of this is almost infinitely small compared to what might be produced by employing the heat directly.*

Hence, there can be no reasonable doubt, that the principal movements of the atmosphere must be owing to mechanical and

* For a detailed account of Lieutenant Maury's speculation, *vide* Edinburgh New Philosophical Journal, vol. li., p. 271 to 292.

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thermotic causes, and only the smaller features to electric and magnotic currents.

A parallel case of the proneness of men to run for an explanation to magnetism, occurred in the early history of the development of the *law of storms*, and has not yet, so far as I am aware, been distinctly refuted by the public, or withdrawn by its promulgator.

In Colonel Reid's first work (1838) on the revolving motion of the hurricanes, after having, in the earlier portion, detailed, in the most satisfactory manner, the *laws of the phenomena*, he gives, in the latter portion, a glimpse of a *theory* of them, or at least, details an experiment in which, on the surface of a magnetised iron shell representing the earth, a rotation in opposite directions was produced in helices in either hemisphere of the ball. This was thought *very interesting*, as the hurricanes are found to revolve in opposite directions in either half of the world; and it was further stated that in St Helena, where the magnetic intensity is small, hurricanes are unknown; while in the West Indies, where hurricanes are so rife, the magnetic intensity is at a maximum.

Here it will be observed, is no attempt to shew whether the magnetic power is *sufficient* to cause the observed effect, or has any power in that way at all, nor even to trace whether this particular coincidence at two points, in the tropical belt of the earth, prevailed at all others also; and in the Colonel's last publication (1848) the question and the experiment are withdrawn altogether.

When, however, we examine the subject more extensively, we find a pretty general rule to prevail all round the world, viz., that hurricanes are most frequent in the western parts of those seas where the trade-wind is suddenly stopped by the occurrence of land, and is unknown in the eastern part of the seas where it begins. Thus, not only is the placid climate of St Helena fully accounted for by being in the eastern position of the South Atlantic, but equally the similar freedom from revolving storms of the Cape De Verd Islands, the NW. and SW. coast of Africa, with California and Peru on the eastern shores of the Pacific.

And again, while the West Indies are pointed out as *likely* places for hurricanes, so are Rio Janeiro, Canton, the Mauritius, and Madras, and, in fact, almost every place where hurricanes have been met with.

The stoppage, then, and interference of the trade-wind, a purely mechanical question, is the cause of the hurricanes, and, according to the greater or less force of the trade-wind, and the greater quantity of air struggling to get over the barrier, as observed in the case of water when a river is in a flood, or on a sea-coast at spring-tide, so are more numerous and more violent eddies found, and they revolve in different directions in either hemisphere, because the direction of the parent trade-wind is also different in each.*

* I have just met with an, at first sight, anomalous instance, in the account

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These mechanical causes, we may be certain, are acting, and must have the chief share in the effects which we observe, and should therefore be followed out in all their consequences, before we attempt to introduce any problematical forces which cannot possibly have much, if they have indeed any effect.*

On the Ethnography of Akkrah and Adampé, Gold Coast, Western Africa. By WILLIAM F. DANIELL, M.D., F.R.G.S., Assistant Surgeon to the Forces, &c. Communicated by the Ethnological Society.

(Concluded from p. 130.)

Architecture, &c.—The towns and villages that lie scattered along the margin of the coast from Cape St Paul's to the Rio Sakkoom westward, exceed, both in size and population, those located in the inland districts. Rocky plateaux or projecting headlands, or eminences situated in the vicinity of the larger salt water ponds or lagoons, were the favourite sites of selection, evidently on account of the two-fold objects which their position commanded, viz., a ready access to the ocean, and a continuous supply of those marine products that would answer either as articles of food or of traffic. From a rude assemblage of fishermen's huts, they, in the course of time, became transformed into places of constant resort, by the progressive development of their commercial resources, and the gradual addition of new habitations, rendered obligatory by the influx of enterprising traders and other people belonging to the circumjacent countries. From the absence of any definite plan or system of arrangement, the erection of the towns was confined within very circumscribed limits; the buildings being so compactly grouped, and in such dense masses as to occupy apparently but a small extent of ground. With the exception of the main thoroughfare and a few open clearances at irregular intervals, the streets were necessarily narrow, tortuous, and intricate; the close proximity of the various domiciles producing a perplexing

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of a circular storm experienced by the American exploring expedition under Captain Wilkes in the neighbourhood of the Cape De Verd Islands, a similar latitude to the West Indies, but on the "wrong" side of the Atlantic, and moreover revolving *with* the hands of a watch, "wrong" also. But the *parent* wind in this case is described to have been *SE.*, which explains everything; and shews that the whole phenomenon is an affair of mechanical conditions in the currents of air at the place; that these being reversed, the hurricane phenomena are reversed also, and that there is no magnetic or other virtue residing in either hemisphere, and compelling air to circulate in any particular direction by reason of its *place*.

* Proceedings of the Royal Society of Edinburgh. Session 1851-2.

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diversity of bypaths, that, in similitude, approached the dubious windings of some mysterious labyrinth. Formed by the contracted spaces between the opposite walls and projecting roofs, their due ventilation and cleanliness was more or less impeded ; consequently, they always continued in a dirty condition, and were likewise subject to that fetid effluvia, generated by the accumulation of filth and other domestic refuse thrown out by their occupants, who, from a constitutional indolency or love of ease, were neither impressed with the necessity of adhering to any sanatory precautions, nor yet endeavoured to obtain the salubrity that would spring from the removal of such morbific agents.

The houses are constructed of swish, a name bestowed on the compositions of mud or other loamy soils, well triturated with water, for such appliances. In style of architecture they resemble the mud cottages which still prevail in most of the rural districts of England. The foundations invariably consist of small fragments of sandstone, embedded in an earthy cement, and elevated two or three feet above the ground, sloping obliquely inwards, so that the base may correspond to the eaves of the roof, and the rain, as it pours from above, may fall on substances sufficiently durable to resist its solvent effects. Upon this elevation the compost is placed in successive layers, each of which is allowed to harden in the sun previous to any further depositions, which continue to be superadded in regular gradation, until the height of ten or fifteen feet has been attained. Its covering is completed by a thatch specially provided for this purpose, whose close adaptation renders it impervious to the heavy torrents of the rainy season. The doors, framework, beams, window sills, and the neat jalousies fitted therein, are executed, with all other wooden fixtures, by native artificers, after European designs, and confer an aspect both of modesty and comfort, which externally assimilates them to the humbler dwellings of more enlightened communities. They are usually built in an oblong or quadrangular form, having an unroofed courtyard in the centre, around which the different compartments of the household are distributed. Should the central area be of such magnitude as to admit of its twofold partition, it is conveniently separated into an inner and outer yard by means of a divisional septum of swish. When this takes place, the latter is allotted to the slaves and family dependents, or portions of it are converted into cookhouses or kitchens, workshops, and other indispensable purposes. The rooms selected for the appropriation of the owner and his near relatives, have, in their internal embellishment, a greater share of consideration devoted to them than the others. The walls are whitewashed, and frequently adorned with coloured prints or coarse engravings, and with a scanty array of home furniture is sometimes intermingled a miscellaneous assortment of foreign articles of a more refined manufacture. An interesting question may here be mooted, whether the peculiar style of architectural configuration at present in vogue among these people,

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claims its derivation from primitive sources, or has been adopted in consonance to the dictates of modern improvements. The result of inquiries will go far to shew the probability of its being an innovation induced by some of those moral revolutions that have terminated in the entire subversion of all preceding conventionalities. It is a remarkable fact that the fetish-houses in every locality are of a circular form, which, owing to the arbitrary doctrines of their religious code or other conventional prejudices, have stood the test of centuries unchanged. Coeval in origin and in similarity of outline, the native tenements may be said to have conjointly descended down the stream of time with them, until the period when the transformation of the former came gradualy into public repute. That such was the case there can be but little doubt, since, within the memory of existing generations, conical mud huts were known not to be uncommon in the suburbs of Akkrah, while in Prampram, Ningo, and other Adampé towns, they are yet to be seen in their pristine simplicity, though fast receding before the progress of what is now considered a more rational system of architecture.

The residences of the white and mulatto merchants and the influential natives, are erected on a much grander scale, and of more expensive materials. Isolated from each other, their snow-like exteriors, and dignified altitude, soon stamped them as the most conspicuous objects of a diversified landscape, and presented at the same time a striking contrast to the low and dusky habitations by which they were surrounded. Composed of stone, hewn from the neighbouring quarries, and wood brought from the colder climates of the north, they, by a skilful subserviency of means, united strength and solidity with comfort and convenience. Built after the commodious plans so prevalent in tropical countries, by having arched balconies or corridors in front and rear, answering not only for pleasant promenades, but serving as a protection against the rays of a fervid sun, and likewise reduced to a mellowed softness the disagreeable glare and temperature that would otherwise pervade the internal partitions. These apartments are lofty, capacious, and well ventilated, and according to the affluence of the inmates, are provided with a sufficiency of domestic luxuries and other ornamental refinements, alone to be found in the higher coteries of civilized life. From two to three storeys in height, with flat roofs, they are in general of large dimensions, containing, independently of other quarters, various wings or enclosures, partially monopolized by the females, junior branches of the family, and their numerous attendants. On the first storey are ranged the reception, dining, and private chambers; and on the ground floor immediately underneath, are those set apart for mercantile purposes and as depôts for foreign and country stores. Connected with the main edifice are several petty outhouses or offices, the whole of which are encompassed by a strong stone wall, varying from 12 to 18 feet in elevation. Within this boundary admission

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is only to be gained by means of a solitary entrance or doorway, sheltered by a porch fitted with wooden benches for the accommodation of those servitors who are attached to the demesne. Although of regular occurrence at Cape Coast, where the aboriginal tenements rise to the altitude of two storeys, here they seldom advance beyond the ground floor, save in a few instances which are to be noticed as exceptions to the general rule. Their compartments are mostly of limited dimensions, and are more or less filthy, from neglect and the accumulation of impurities.

In proximity to Jamestown, Christianburg, and Prampram, may be observed separate salt water lakes, each of which are distinguished by certain appellations; those in the environs of the first two towns are recognised by the terms of *Kualé* and *Clorté*, and from superstitious motives are deemed sacred. Of the three, that of Jamestown or English Akkrah is the most extensive. All teem with an abundance of crabs, shell-fish, and a species of small round fish extremely prolific, the young fry of which are eaten with avidity; and from their rapid reproduction, compensate the poorer classes for that deficiency in similar kind of food to which their poverty subjects them. To each of these towns is also appended a reservoir of fresh water, which, during the prevalence of the rains, is always filled to its full extent; but from subsequent use and constant evaporation, the fluid eventually becomes diminished to one half, and for the greater part of the year remains in a stagnant and impure state; nevertheless it is exclusively retained, from the facility it affords for personal ablutions and purification.

Forming a direct communication between the three Akkrahs and the rural hamlet of Fredericksburg, are roads, maintained in excellent order chiefly through the exertions of the European residents. Portions of them are fringed at intervals by the tamarind, chashew, and other ornamental trees; while in several of the suburban avenues are planted rows of the *Hibiscus populneus* and a species of *Ficus* or umbrella tree, so designated from the umbrageous canopy which its leaves produce. On the verge of the footpaths that radiate from the outskirts on different sides may be met the indigo, castor-oil, and cotton shrubs, with fences of *Cacti* and *Euphorbæ* even as the magnificent *Bombax* flourishes amid the masses of human habitations, in conjuction with the tapering coco-nut tree, that waves its feather-like branches o'er the precincts of the same dwellings, as if in grateful acknowledgment of the tender nature which their protection yielded to its early growth. The streets and thoroughfares of the Adampé town and villages are stated to be much superior to those of Akkrah, being more cleanly, spacious, and of uniform width.

Markets.—Markets are held on every day of the week, save on such as are dedicated to religious observances. The situations usually adopted are either at the entrance or termination of one of the principal streets adjoining some cleared space of ground, or in

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localities habitually frequented by a concourse of people. Occasionally the stray exhibition of a few articles may be noticed opposite the domiciles of the vendors, or along the walls in the more secluded passages. Compared to similar places of resort elsewhere in Western Africa, they present an impoverished appearance, from the meagre pittances of food and other indigenous products which are offered for sale in such limited quantities. The whole are vended under the patient instrumentality of women and children, who, squatted in regular lines along the sides of the streets, or beneath the shade of the adjacent houses, dispose their effects to the greatest advantage, in assorted lots, spread out upon mats or in calabashes, around the spot on which they are stationed. These collocations of edibles and other necessary articles, for the most part comprise plantains, bananas, peppers, limes, oranges, ground nuts, Malaguetta pepper, native soap, pine apple, and other kinds of flax, tobacco cut in small pieces, ochros, dried and fresh cassada, kankies baked or boiled, and other preparations of maize, pine apples, soursops, a few miraculous berries, shallots, palm oil, and shea butter, kola nuts, dried and fresh fish, smoked deer, and goats flesh, &c., with beads, earthenware, chintzes, ramals, guns, copper basins, and a variety of native and foreign cloths, suspended on lines attached to the different houses above the heads of the anxious dealers, &c.

Harvest Festivals.—The great annual festival of the Akkrahs termed *Homowaw*, is one celebrated with much pomp and dissipation. Numerous and important are the ceremonies enacted on these memorable holidays, and multiform are the scenes that attest the vigour and exultation of their commemoration. By every family in town or country proparations on a proportionate scale are carried into effect long antecedent to the period of their commencement, which in general occurs early in the month of September, Friday being the day that announces their wished-for arrival. In the year 1850 the anniversary fell on the 6th of September, and the peculiar observances attending the initiation were of the same determinate character as those on previous occasions. The ordinary duration of these popular orgies seldom exceeds ten days or a fortnight (a week being the allotted term of fulfilment); but should a continuous supply of potables, and other accessory stimulants, be furnished, or as long as they possess the means to purchase them, their prolongation is carried on with undiminished vigour, until it finally ceases, from an exhaustion of their pecuniary resources. According to the reports of residents and other local authorities, this particular season has been consecrated by the blending of various religious and social rites; a series of aggregate concessions that portray the worship of many barbarous races, when offering their grateful adulations to a Supreme Intelligence, not only for the benefits conferred during the past, but for the prosperous endowment of the approaching year. From the semblance considered to exist between them and

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those hospitable entertainments of Europeans in their own country, though at another season, it has acquired the designation of the Akkrah "Christmas." On Soah, the first day of its celebration, the *Occhās* and other influential personages of the town, bestow liberal donations of cloth, beads, and other desirable articles, on their wives, families, and near relatives; and at the same time, transmit to their patrons and respective fathers-in-law a large log of wood, which to the latter is an acknowledgment of their consanguinity. The door-frames, window-sills, and other wooden work of the houses, are now partly covered with a red ochre, and in honour of the dead their family graves are equally adorned by the same florid colour. In former years a thorough purification of the houses, with other sanatory measures, appear to have been instituted; but latterly, this and the preceding custom are imperceptibly falling into disuse, and doubtless ere long will become obsolete.

During the continuance of this festival a remission of all public business occurs, and the daily avocations of the labouring classes are almost suspended, one predominant train of thought alone pervading every grade, both high and low, rich and poor, viz., the unlimited gratification of their passions, and an anxious determination to avail themselves of every opportunity for self-indulgence which this interval of jollity and relaxation can afford them. The men, dressed in their best attire, with fillets of cloth or twisted handkerchiefs encircling their heads, parade through the town in noisy communities, accompanied with drum and horn; and, as if mimicking the bacchanalians of old, exhibit the most equivocal dances and grotesque attitudes. The women, left to their own resources, assemble in picturesque groups, and, like the men, express a similar delight in the participation of these enjoyments; they also perambulate the streets, visit their friends and connections, and elaborately decorate themselves in their favourite costumes of silk and chintz. Gold rings and chains, fancy beads of every hue, bracelets, and armlets of divers construction, with the conspicuous aid of white and yellow figures or patches of paint, to ornament the features, contribute to gratify their self-esteem, and sufficiently testify to their love of finery, desire of conquest, and that inherent vanity characteristic of the sex.

Among the men, intoxication, committed to excess, from copious libations of rum, constitute in their estimation, the *summum bonum* of happiness; and they who have not the means of thus distinguishing themselves, when passing abroad or elsewhere, conceal their poverty by carefully imitating the gait and erratic vagaries of their drunken compeers. In conformity with the primitive ordinances of the country, a species of large fish named *Chillé*, caught at this period of the year, and until now prohibited from public use by the fetishmen, furnishes the chief constituent in their palm oil and other soups, being eaten with a certain pudding, or rather meal, termed *Kou*, made from ground maize mixed with palm oil and a

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few ochros. At this season these edibles obtain a temporary preference beyond others; and since some care and trouble is lavished in their culinary preparation, they naturally become the favourite dishes, which all ranks seek and partake of with avidity.

On Saturday or Hau, the termination of the old year, oblations are offered to the manes of their ancestors: portions of the preceding kinds of food being placed around their graves in the different compartments of the mansion.* Haughbah or Sunday is the most venerated, on account of its being the first day of the new year, the birth of which is ushered in by a strange medley of congratulations and laments, the latter more exclusively emanating from the female sex, who, with pathetic exclamations and a profusion of tears, bewail those members of the family who, during the intervening period between the past and present custom, have departed this life for the regions of another world.

About this time the congenial rehearsals of feasting and dissipation attain their zenith, and although their most disgusting features are seldom openly displayed, yet, within the walls and inner courts of the larger domiciles, the vociferous chanting, boisterous mirth, and clamorous bickerings of their intoxicated inmates, bear ample testimony to the dissolute revels performed therein. To the philosophical observer, these indications of moral degradation create melancholy reflections, and excite in him impressions of painful surprise, how a people like the present, after the lapse of so many centuries, should have so partially emerged from the depths of primitive barbarism, when endowed with these important advantages that accrue from an eligible position, fertile country, and the intimate alliance with more enlightened Europeans who have resided so long amongst them, and have constantly reciprocated their commercial wants for so great a number of years.

The Tuesday following is a day more exclusively dedicated to the performance of certain religious ceremonies to which the natives are much addicted; and as they are more or less interpolated with most other public festivities, they, in general, compose the most solemn and impressive portion of them. By all grades of people, therefore, a considerable amount of deference and awe is paid to these superstitious observances, inasmuch as they believe that some mysterious

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* A similar custom was observed by the Romans, on the celebration of their feasts, called *Silicernia*, in which food was provided for the dead, and deposited on their graves. It is alluded to in Ovid. de Fastis, lib. 2, 533, as follows:—

> "Est honor et tumulis, animas placate paternas;
> Parvaque in extinctas munera ferte pyras.
> Parva petunt manes, pietas pro divite grata est
> Munere, non avidos Styx habet ima Deos.
> Tegula projectis satis est velata coronis;
> Et sparsæ fruges, parcaque mica salis:
> Inque mero, mollita Ceres, violæque solutæ."

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potency originates from them, which has been supposed to exert a specific influence, either for good or bad, over the future career of those that become suppliants for their protection, or fail to offer the requisite degree of propitiation. The peculiarity of this mode of worship is chiefly characterised by ablutions of the whole body with water, which has been previously sanctified by the priests, and in which the leaves of some plant have been steeped either in the fetish or their own houses. To this liquid they attribute manifold prophylactic virtues, and, from its reputed efficacy, they imagine that exemption from death or other dire misfortunes is thus secured for the ensuing year; through the interposition of the deity whose all pervading power they have submissively invoked. During the exhibition of these sacred observances, the fetishmen reap a bountiful harvest, as a compensation for their successful predictions, and the labours they now incur; for when any individual, with his wives or children, require these abluent purifications, or become desirous of gaining an insight into the depths of futurity, the request is always accompanied by a regulated fee, proportionate to his position in the country. The prices, therefore, fluctuate from a few strings of cowries or bottles of rum to other articles several dollars in value. From the peculiar rites that characterise this day, it has obtained the appellation of the Sakkoom fetish-day.

In Ossu and Labadde these holidays commence about ten days subsequently to those in English and Dutch Akkrah, and, like them, are maintained with equal energy and display. With the two former there is merely this difference, that the first day of their inauguration is invariably held on a Wednesday, in conformity to the ancient regulations of these localties.

Defence of the Doctrine of Vital Affinity, against the objections stated to it by Humboldt and Dr Daubeny. By Dr ALISON.

The object of this paper was to fix attention on the great physiological discovery which has been gradually effected during the present century, of the mode in which certain of the elements contained in the earth's atmosphere, under the influence of light and of a certain temperature, are continually employed in maintaining that great vital circulation, of which vegetable structures, animal structures, the air, and the soil, are the successive links; and to point out that the most essential and fundamental of the changes here effected—particularly the formation of the different organic compounds in the cells of vegetables,—are strictly *chemical changes*, at least as clearly distinct from any chemical actions yet known to take place in inorganic matters, as the vital contractions of muscles are distinct from any merely mechanical causes of motion; and justifying the statement of

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Dr Daubeny, that there appears to be a "power, residing in living matters" and producing chemical effects,—in fact manifesting itself most unequivocally by the chemical changes which result from it,—"distinct, at least in its effects, from ordinary chemical and physical forces."

But after having made this statement, Dr Daubeny, according to the author of this paper, has thrown a degree of mystery over the subject which is quite unnecessary and even unphilosophical, by refusing to admit—and quoting Humboldt, who has changed his opinion on the subject, and now likewise declines to admit—that these changes are to be regarded as *vital;* both authors (as well as several other recent English authors) maintaining, that as we do not know all the conditions under which ordinary chemical affinities act in living bodies, we are not entitled to assert that these affinities may not yet be found adequate to the production of all the chemical changes which living bodies present; and that until this *negative proposition* is proved, it is unphilosophical and delusive to suppose the existence of any such power, as that to which the term Vital Affinity has been applied by the author of this paper and several other physiologists.

In answer to this, it is here stated, that as we cannot strictly speaking, *define* Life or Vitality, we follow the strict rules of philosophy, in *describing* what we call living bodies, whether vegetable or animal, and then applying the term Vital or living, as the general expression for everything which is observed to take place only in them, and which is inexplicable by the physical laws, deduced from the observation of the other phenomena of nature; that according to this,—the only definition of which the term *vital* admits, or by which the objects of Physiology can be defined,—Dr Daubeny has already admitted, in the expressions above quoted from him, that chemical as well as mechanical changes in living bodies, fall under the denomination *vital;* and as the rule of sound logic is "*affirmantibus* incumbit probatio,"—and as it is just as probable *à priori*, that, with a view to the great objects of the introduction of living beings upon earth, the laws of chemistry, as those of mechanics, should be modified or suspended by Almighty Power,—this author maintains that we are as fully justified in referring all great essential chemical phenomena, which are peculiar to living bodies, to peculiar affinities, which we term vital, as Haller was to ascribe the peculiar mechanical movements of living bodies to the vital property of Irritability; and to throw on the mechanical physiologists of his day the burden of proving, if they could, that the laws of motion, perceived in dead matter, were adequate to explain them.

In illustration of the importance, both in Physiology and Pathology, of this principle being held to be established, Dr Alison adduced two examples, *first*, the utter failure of the very ingenious theory of Dr Murray to explain, on ordinary chemical principles, the simplest and most essential phenomena of healthy Secretion; and,

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secondly, the now generally admitted inadequacy of any theory of Inflammation, which does not regard a modification of the *affinities* peculiar to life, and here termed vital, as the primary and essential change, in the matter concerned in that process.*

On the Blood-proper and Chylo-aqueous Fluid of Invertebrate Animals. By THOMAS WILLIAMS, M.D.

In this paper the author, as stated in the Official Report on it in the Proceedings of the Royal Society for March 1852, has accumulated numerous observations, founded upon dissection and microscopic inquiry, to prove that there exist in *invertebrate animals* two distinct kinds of nutrient fluids; that in some classes of this subkingdom, these two fluids co-exist in the same organism, though contained in distinct systems of conduits, while in others they become united into one. The author proposes to distinguish these two orders of fluids under the denominations of the *blood-proper* and *chylo-aqueous fluid*. The former is always contained in definitively organised (walled) bloodvessels, and having a determinate circulatory movement; the latter with equal constancy, in chambers and irregular cavities and cells communicating invariably with the peritoneal space, having not a determinate circulation, but a to-and-fro movement, maintained by muscular and ciliary agency. He then adduces evidence, derived from dissection, in proof of the statement that the system of the blood-proper does not exist under any form, the most rudimentary, below the Echinodermata; that, in other words, the system of the true blood, or of the blood-proper, begins at the Echinodermata. The author then shews that below the Echinodermata, namely in the families of Polypes and Acalephæ, the digestive and circulatory systems are identified, and that consequently the external medium is admitted directly into the nutrient vessels. He considers that this circumstance constitutes a fundamental distinction between the chylo-aqueous system and that of the blood-proper, into which, under no conditions, is the external inorganic element directly introduced.

He conceives that his observations suffice to establish the law, with reference to the chylo-aqueous fluid, that in every class in which it exists, it is charged more or less abundantly with organised *corpuscles*. This is an invariable fact in the history of this fluid. His inquiries shew that these corpuscles are marked by distinctive microscopic characters, not in different classes and genera only, but in different species, entitling these bodies to great consideration in the establishment of species.

The paper then proceeds to demonstrate the proposition, that in

† Proceedings of the Royal Society of Edinburgh. Session 1851-2.

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those classes, as in the Echinodermata, Entozoa, and Annelida, in which, in the adult animal, these two orders of fluids coexist, though distinct, in the same individual, there prevails between them, as respects their magnitude or development, an inverse proportion: that while, as instanced in the Echinoderms, the chylo-aqueous fluid filling the ciliated space between the stomach and integument is *considerable* in volume, the blood-proper and its system are little evolved; that while, as in the Entozoa, the chylo-aqueous fluid is still the most important fluid element in the organism, the blood system is proportionally rudimentary; that in the Annelida, especially the higher species of that class, the chylo-aqueous fluid almost disappears, while the system of the true blood acquires, illustrating the law of inverse proportion, a correspondingly augmented development. The author then states, that the system of the chylo-aqueous fluid does not exist in the *adult*, but only in the larva state of the higher members of the articulated series, such as the Myriapoda, Insecta, and Crustacea.

In myriapods and insects, he has observed that the peritoneal space is occupied by a fluid which does not communicate with, and is distinct in composition from, the contents of the true bloodvessels.

This peritoneal fluid, however, in these classes, disappears at a subsequent stage of growth. Thus the author thinks, that a continuous chain *through the medium of the fluids*, is established between the Echinoderms at one extreme, and the Crustacea at the other. These classes he proposes to connect together under the designation of the *double fluid series*, corresponding to the radiate and articulate series of systematic zoologists.

Returning to the standard of the Echinoderms, where the system of the blood-proper first appears in the zoological scale, he shews that at this point the *Molluscan* chain diverges from the radiate and articulate chain, and may be indicated in contradistinction from the latter, as the *single-fluid series*. The author's observations lead him to believe, with Professor Milne-Edwards, that in all Molluscs, from the Tunicata to the Cephalopods, the chamber of the peritoneal is continuous with the channels of the circulation, and that consequently the fluids observed in these parts, are *one and the same fluid*, establishing the *singleness* of the fluid system of the body; and this conclusion is corroborated by additional evidence drawn from microscopic examinations.

He then recapitulates the results of his researches, and maintains that the base of the invertebrated kingdom of animals is formed of all those inferior series which rank below the Echinoderms; and that this series is distinguished from the Molluscan, in which also the fluid system is single, by the important circumstance that in the former, unlike the Mollusca, the digestive and circulatory systems are identified, or confounded into a single system; that at the Echinoderms, the series divaricates into the double-fluid series and single-

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fluid series, the former coinciding with the radiate and articulate class, and joining the Vertebrata through the Crustacea; the latter running parallel with the Molluscan order, and connecting itself to the Vertebrata through the Cephalopods.

The fluids of the zoophytic series are invariably corpusculated, but the corpuscles cannot yet be reduced to any definite type of conformation. In the Medusan series, these bodies become more definitively organised. The author then demonstrates, that throughout the whole radiate and articulate classes, wherever it is found, the *chylo-aqueous* fluid is richly corpusculated, or in other words, charged with floating morphotic elements, which, from the constancy of their characters in different species, become grounds for specific distinctions. It is stated, that, throughout the Echinoderms, Entozoa, and Annelida, in which, even in the adult animal, the blood-proper and the chylo-aqueous fluid, though separate, coexist, the latter fluid only is corpusculated, the true blood being invariably limpid and perfectly fluid (incorpusculated), and almost always the seat of the colour, the latter existing as a substance dissolved in the fluid; while in no instance does colour develop itself in the chylo-aqueous fluid.

The paper then shews, that at the point where the chylo-aqueous system disappears, namely, at the Myriapods, the true blood becomes the vehicle of the corpuscles.

And lastly, the author adduces a great variety of observations in confirmation of the statement, that throughout the whole Molluscan series without exception, coinciding with his "*single-fluid series*," the fluids are richly charged with corpuscles.

The Future of Geology.

A system in Geology is like a genus in Zoology and Botany,—an arbitrary division for one person, an attempt to express a natural group for another and more philosophical head. It is consequently a term of very different value in the writings of one geologist, to that which it enjoys in the works of another. What one calls a *formation*, his neighbour calls a *system*. Yet each, if he be aiming, as we presume is the purpose in most instances, at the establishment of a natural group in time, is really treating of the same order of thing, with a difference only in degree. It would be desirable, doubtless, that we should have one uniform terminology; but the age is not ripe for such an invention yet. We are working towards it, but must not hurry. An idea is gradually being evolved out of the efforts at the discovery and defining of geological subdivisions. It is the idea of *facies*. The faunas and floras that have succeeded each other, interlacing in their succession, during the course of geological ages, exhibited from time to time peculiar combinations of forms, affinities, and analogies, which,

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taken in their totality, imprinted a recognisable *facies* on each assemblage of organisms—on the population of the earth and sea during successive epochs or groups of ages. When a palæontologist is shewn a number of unknown fossils from a distant and unexplored country, he recognises almost instinctively an aspect in the collection which induces him to declare them, with little hesitation, to be palæozoic, or oolitic, or whatever the term may be. The pure geologist seizes with avidity on this determination, and assigns to the rocks from whence the specimens have been obtained a definite position in his scale of formations. If the naturalist recognise a previously determined species, his colleague is the more certain of his decision, and from the shell or plant, or coral, that is known, decides upon the age of the rock and region that are unknown. Nevertheless, it is more than questionable whether identity of species in two or more very distant localities should imply synchronism of age of the strata wherein they occur; indeed, it is less than improbable that the inference drawn from the fact should be exactly the reverse. If so, what becomes of the hard horizontal lines drawn on our tables and diagrams between systems and formations? They have been assumed, it is true, from the consideration of facts—but facts of a *local* character, and important only in connection with *limited regions*. In reality we are often endeavouring to apply a scale which, in its subdivisions, is true only in Britain and part of Europe, to the whole world. The procrustean operation has been too often performed by geologists.

Regarding, then, the geological scale of formations as an artificial scheme, founded on local considerations, although an instrument and standard of comparison of great value when used judiciously, the questions have still to be answered, which demand whether the terms of its graduation be required, and whether, such as we have them, they are complete. There are reasons for believing that they are far from being so, and that future research will intercalate many unrecognised stages.

See those broad stripes of demarcation painted on every geological diagram between the terms palæozoic and secondary, secondary and tertiary. Those lines are popularly understood to mark the boundaries between a complete cessation of one great system of types of species, and the commencement of an entirely new series of creatures, animal and vegetable. They really mark prodigious gaps in our knowledge of the sequence of formations and the procession of life. One of those supposed impassable barriers or boundaries, that between "tertiary" and "cretaceous," threatens rapidly to give way, and to vanish in due time as speedily as artificial social distinctions in society. In France, in Germany, in Belgium, in England, there are symptoms of an intergrowth between the long-separated "chalk" and "eocene." Strata are coming to light which rudely insist on finding elbow-room among our neatly-packed systems and formations. Janus-like

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fossils are turning up with two sets of features. Our preconceived notions of what ought to be, are sadly disconcerted. An already extensive terminology is threatened with an inundation of new terms, too necessary to be evaded.

If we are not greatly mistaken, there are little clouds rising on the geological horizon that indicate revolutions elsewhere in the series. That narrow black line drawn on geological diagrams between the words "Trias" and "Permian" has more meaning in it than its thin dimensions indicate. The line between "Eocene" and "Cretaceous" has swollen out, broken up, and is enlarging fast into intermediate sections. But all its changes and increase will be as nothing compared with those which must take place by and by, in its representative lower down. If we interpret aright the signs indicated by extinct organisms preserved to us in palæozoic rocks, and the comparison of them with others contained in the lowest mesozoic or secondary strata, there is a gap in our knowledge of the succession of formations, the extent of which it is almost disheartening to think upon. Although the palæozoic fauna and flora are assuredly portions of the same unique system of organised nature with the assemblages of creatures of after-date in time, they exhibit differences in detail so great that, on superficial consideration, we might almost be inclined to regard them as belonging to some other world than our own. These differences are such as at present set all our calculations respecting the climatal conditions of the primeval (palæozoic) epochs at defiance. But that these oldest of creations were linked with those that came after, and those amidst which we live, is evident in the number of generic types common to all, and expressed yet more strongly in the presence of straggling representatives of types of life, characteristically palæozoic, among the very lowermost strata of the secondary period. All analogy, however, teaches us that there is a graduation of one geological epoch into another; and every day's advance in research goes to confirm this belief. The facts to which we have alluded indicate evidences of such a graduation of palæozoic into secondary. But the stages of that graduation, the intermediate formations, have not yet been discovered. Calculating from the amount of the blank in the series of organised types, there must have been a vast interval of time intervening between the Permian and Triassic epochs, during which, doubtless, sediments were being deposited in seas, sea-beds upheaved, animals and plants flourishing, generations and generations, nay more, creations and creations (we use the popular and hypothetical term, for want of a better), appearing, succeeding, and disappearing; and yet of all these mineral accumulations, and organised assemblages, there has not been as yet a fragment found.

"They are but ill discoverers," wrote Lord Bacon, "that think there is no land when they can see nothing but sea." Columbus had fewer signs to warrant his belief in a new continent than we have to indicate an unexplored, and as yet unseen, geological world.

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Such signs cannot be dissipated by any appeal to the series of strata already investigated. The answer to that appeal would be favourable to our hypothesis: moreover, in the present state of our knowledge of comparative geology, it would be folly to claim infallibility for geological scales founded upon the examination, partly minute, partly superficial, of regions chiefly confined to the land of the northern hemisphere. If we jot out on the map of the world those portions which have been sufficiently examined, at once palæontologically and geologically, the space covered by our ink makes but a poor show; yet only about such districts can we lay claim to sufficient knowledge,—if, indeed, knowledge be ever sufficient. Our hope lies in the rapidly-advancing progress of comparative geology, especially through the aid and sure operations of organised surveys. All over Europe, such surveys are in progress, or about to commence, sanctioned, as they ought to be, by governments of every shade of opinion.

Some three or four years ago it was publicly declared that the geology of England was completed; a plausible announcement, since almost every corner of the country had been subjected to the tramp and hammers of geologists. Yet, if we are not greatly mistaken, even the geology of England has still to be done. It is ably sketched out; portions of it have been developed with skill and ability;* but by far the greater part will yield a luxuriant harvest of discovery to those able and willing to enter upon the task. Compare any sheet of the Ordnance map of England, after it has been re-issued, with the geology laid down upon it by the Government surveyors, with any pre-existing geological map of the district, and see there what an amount of fresh detail has been educed by the patient labour and unhurried explorations of those geologists to whom, under the superintendence of Sir Henry de la Beche, the work is due. The economical value of geological researches depends mainly on such works. The nearer we come to geologizing by square miles or leagues the more interesting will be the results of our labours. Ages, however, must elapse before we can hope to obtain similar results from all countries of the earth. And yet until we do, not even the geology of England, small though it be, can fairly be said to be completed; for not until we have obtained a full and minute knowledge of comparative geology, can we understand clearly one-half the facts and phenomena exhibited in the structure of any country, however limited, in the world.—*Vide Westminster Review, July* 1852, for further expansion of this topic.

* The geognostical characters of the Old Red Sandstone, the so-called Devonian or Caledonian group,—of the Greywacke or Silurian system,—of the Clay Slate or Cambrian system, are still under discussion in England.—*Editor.*

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Divisibility of Matter.

Many years ago, a curious calculation was made by Dr Thomson, to shew to what degree matter could be divided, and still be sensible to the eye. He dissolved a grain of *nitrate of lead* in 500,000 grains of water, and passed through the solution a current of sulphuretted hydrogen, when the whole liquid became sensibly discoloured. Now a grain of water may be regarded as being about equal to a drop of that liquid, and a drop may be easily spread out so as to cover a square inch of surface. But under an ordinary microscope, the millionth of a square inch may be distinguished by the eye. The water, therefore, could be divided into 500,000,000,000 parts. But the lead in a grain of nitrate of lead weighs 0·62 grain; an atom of lead accordingly cannot weigh more than $\frac{1}{310,000,000,000}$th of a grain, while the atom of sulphur, which in combination with the lead rendered it visible, could not weigh more than $\frac{1}{2,015,000,000,000}$, that is the two billionth part of a grain.

But what is a billion, or rather, what conception can we form of such a quantity? We may say that a billion is a million of millions, and can easily represent it thus:—1,000,000,000,000. But a schoolboy's calculation will shew how entirely the mind is incapable of conceiving such numbers. If a person were able to count at the rate of 200 in a minute, and to work without intermission twelve hours in the day, he would take to count a billion 6,944,444 days, or 19,025 years 319 days. But this may be nothing to the division of matter. There are living creatures so minute, that a hundred millions of them might be comprehended in the space of a cubic inch. But these creatures, until they are lost to the sense of sight, aided by the most powerful instruments, are seen to possess organs fitted for collecting their food, and even capturing their prey. They are, therefore, supplied with organs, and these organs consist of tissues nourished by circulating fluids, which circulating fluids must consist of parts or atoms, if we please so to term them. In reckoning the size of such atoms, we must speak not of

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billions, but perchance of billions of billions. And what is a billion of billions? The number is a quadrillion, and can be easily represented, thus: 1,000,000,000,000,000,000,000,000; and the same schoolboy's calculation may be employed to shew that to count a quadrillion at the rate of 200 in the minute, would require all the inhabitants of the globe, supposing them to be a thousand millions, to count incessantly for 19,025,875 years, or for more than 3000 times the period for which the human race has been supposed to be in existence.—*Professor Low.*

On two New Processes for the detection of Fluorine when accompanied by Silica; and on the presence of Fluorine in Granite, Trap, and other Igneous Rocks, and in the Ashes of Recent and Fossil Plants. By GEORGE WILSON, M.D.*

In several communications made to this Society and to the British Association, I have announced the results of a series of observations on the distribution of Fluorine throughout the mineral, vegetable, and animal kingdoms. To myself, the least satisfactory part of these investigations has been the inquiry into the presence of fluorine in plants, for I have been more frequently foiled than successful in my attempts to detect it in them. Others have not, apparently, been more successful. Daubeny was as unable as Sprengel at an earlier period had been, to obtain evidence that the element under notice is present in vegetable structures; and Will of Giessen, the discoverer of fluorine in plants, speaks only of "traces" of it having been detected in barley. Later observers have not spoken more confidently concerning its abundance in vegetables; and in the many analyses of the ashes of plants which have recently been published, it seldom, if ever, finds a place.

That one cause of this apparent rarity of fluorine in vegetables, is the small extent to which it occurs in them is certain; but I have never doubted that the chief reason why it

* Read before the Royal Society of Edinburgh, April 19, 1852.

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appeared to be so scanty a constituent of plants, was its occurrence along with silica, which makes its recognition very difficult. I had given up, accordingly, all hopes of satisfactorily demonstrating its wide distribution, till better processes than are at present in use, were devised for its detection when accompanied by silica.

For the same reason I have thought it hitherto useless to endeavour to trace back fluorine from the plants, animals, natural waters, and the more accessible strata which are the main seats of life at the present day, to those earlier rocks and geological formations which have furnished our soils, and have contributed the chief soluble matters which are found in the lakes, rivers, and seas of the globe. The more ancient rocks abound in silica, and, with our present processes, the prospect of discovering fluorine in trap and similar siliceous masses, was not encouraging. A representation, however, from Professor Jameson, as to the importance attaching to the detection of fluorine in the most ancient rocks, led me to reconsider the geological and mineralogical interest which the inquiry possessed; and within the last six weeks I have put in practice two methods of investigation, which I shall now explain.

The processes at present in use for the separation of fluorine from silica, are in many respects satisfactory; but they imply the rejection of glass apparatus, and the use of vessels of platina, which, from their costliness, cannot be employed of any considerable size, and, from their opacity, render the observation of phenomena occurring within them impossible. They are thus inadmissible for operations where large quantities of materials must be dealt with: and to the impossibility of employing glass and porcelain vessels, must be largely attributed the comparatively limited extent of our information as to the distribution of fluorine.

The following processes, which, in the meanwhile, are offered only as qualitative (although I hope to succeed in rendering the second of them quantitative), may be carried on in the ordinary glass and porcelain vessels of the laboratory, and admit of everything visible being observed. They are applicable to all siliceous compounds or mixtures contain-

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ing fluorine, provided it be present in the form of a fluoride which admits of decomposition by oil of vitriol at its boiling point. The first stage of the process consists, in both cases, in heating the silicated fluoride in a flask along with strong sulphuric acid, so as to occasion the evolution of the fluoride of silicon, $Si F_3$. This gas is conducted by a bent tube into water, where it deposits a portion of gelatinous silica; and the liquid, after filtration (which, however, is not essential), is treated as follows:—

In the first process, I adopted one of Berzelius' well-known methods for the isolation of silicon. The filtered liquid was neutralised with potass: and the resulting gelatinous precipitate of fluoride of silicon and potassium ($2 Si F_3 + 3 KF$), after being washed, was dried, and transferred to a small metallic crucible, in which it was heated with potassium, so as to separate and set free the silicon, and convert the whole of the fluorine into fluoride of potassium. This fluoride was then dissolved out by water, evaporated to dryness, and treated in the ordinary way with oil of vitriol, so as to evolve hydrofluoric acid, which could be made to record its evolution by the etching which its vapour occasioned on a plate of waxed glass, with lines written on it through the wax.

This process is necessarily tedious, and is liable to several objections. The most serious of these is the impossibility of effecting the complete decomposition of the fluoride of silicon and potassium, by potassium, so as to liberate the whole of the silicon; and the risk of the latter undergoing oxidation into silica during the washing of the ignited mass. Accordingly, though this method gives good results, and has enabled me to detect fluorine in coal, in which I could not previously detect more than the faintest traces of it, yet it almost unavoidably necessitates a loss of the element in question, and is much inferior in simplicity and certainty to the process which I am about to describe.

In the second process, as in the first, the substance under examination is heated with oil of vitriol, so as to yield fluoride of silicon, which is conducted into water. The resulting solution (with or without filtration) is neutralised with ammonia instead of potass, and then evaporated to dryness,

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which has the effect of rendering the silica produced insoluble. On digesting water on the residue, fluoride of ammonium is dissolved, and the solution requires only to be evaporated to dryness and moistened with sulphuric acid to give off hydrofluoric acid, which readily etches glass. The stages in the ammonia process are thus :—

1*st*, Distillation of the substance with oil of vitriol, so as to produce fluoride of silicon, $Si F_2$.

2*d*, Neutralisation of the aqueous solution of the distillate with ammonia in excess, so as to produce fluoride of silicon and ammonium, $2 Si F_2 + 3 NH_4F$.

3*d*, Evaporation of the neutralised liquid to dryness, so as to separate silica and render it insoluble.

4*th*, Exhaustion of the residue with water, and evaporation to dryness, so as to leave fluoride of ammonium.

5*th*, Moistening of the ammonio-fluoride with oil of vitriol, so as to liberate hydro-fluoric acid, which will act upon glass.

I have tried this process with Aberdeen and Peterhead granite; with three trap rocks from the neighbourhood of Edinburgh, namely, basalt from Arthur Seat, greenstone from Corstorphine Hill, and clinkstone from Blackford Hill; with a deposit from the boiler of the Atlantic steamer, Canada; with a fossil bone; with the ashes of charcoal, of barley-straw, and of hay; and in all with such success, that the applicability of the process to the end proposed is certain. The pieces of glass, etched by hydrofluoric acid evolved from the substances referred to, which I lay upon the table, are not selected successful specimens, but represent the whole of the trials made by the ammonia process. The etchings on the majority of them are as deep as could be obtained from pure fluorspar and oil of vitriol; and, with the experience which I have now acquired, I have no doubt that I shall be more successful in succeeding trials with vegetable ashes, which, for reasons to be presently mentioned, require more precautions than fragments of rock do.

The examination of a hard crystalline mineral, such as granite or an unweathered trap, presents no difficulties. It must be reduced to a tolerably fine powder, and employed in considerable quantity. A little sulphurous acid is always

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evolved during the action of the oil of vitriol, from the dust which is gathered during a protracted process of powdering; but the presence of this acid in small quantity is of no importance, and the powdering of the rock is the most troublesome part of the investigation.

It is otherwise with weathered granite and trap, which contain chlorides and carbonates, and give off hydrochloric and carbonic acids when treated with sulphuric acid. These gaseous acids materially interfere with the processes described by the frothing which they occasion, and by their tendency to sweep away the hydrofluoric acid which may accompany them. In my earlier trials, accordingly, I treated the powdered pieces of rock with hydrochloric acid, and washed them with water, then dried them, and heated them with oil of vitriol. The preliminary treatment, however, risked, and, I have no doubt, occasioned, the loss of the fluorides present in the mineral, which were soluble in water or in hydrochloric acid, and latterly I abandoned this process. I refer to it here only because it explains certain of the less perfect etchings which are exhibited.

In later trials, a simpler and more satisfactory process has been put in practice. The powdered rock has been added to oil of vitriol in the cold, in small quantities at a time, so as to prevent any great rise in temperature. So long as the heat evolved is not considerable, there is no risk of fluorine escaping, either as hydrofluoric acid or as fluoride of silicon, whilst any chlorides or carbonates present are decomposed, and the hydrochloric or carbonic acids evolved are carried away before their escape can interfere with the evolution of fluorine. When the oil of vitriol is afterwards raised to its boiling point, the fluoride of silicon is liberated, and little difficulty attends its collection and identification.

The ashes of plants are somewhat less easily examined. They almost invariably contain charcoal, which occasions the evolution of sulphurous acid with hot oil of vitriol. Sulphurous acid, however, does not very materially interfere with the detection of fluorine, as it can be expelled by heating the distillate before adding ammonia, which is the process I have hitherto generally followed. It may also be

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converted into sulphuric acid by the cautious addition of nitric acid, and then its presence is quite immaterial. But in several quite successful trials no steps were adopted to separate the sulphurous acid.

The specimen laid upon the table, of glass etched by fluorine from barley-straw, will illustrate the applicability of the process to plant-ashes largely charged with silica, and which yielded with oil of vitriol, carbonic and hydrochloric acid, besides much sulphurous acid.

The glass etched by the fluorine of charcoal-ashes is still more deeply corroded, although they were subjected to no preliminary process to remove the volatile acids which they contained, or to set free or separate the sulphurous acid which they yielded.

In truth, the ammonia process has succeeded with every substance upon which I have tried it. The worst result has been with the ashes of hay, but they had been washed with water and hydrochloric acid to remove chlorides and carbonates; and in former papers I have shewn that such washings remove fluorides. Notwithstanding this, the evidence of the presence of fluorine in hay, afforded by the specimen, is such as has not hitherto (so far as I am aware) been afforded by any analyst, and the omission of the washings will, I have no doubt, yield a still more satisfactory result on a repetition of the analysis. The same remark applies to coal-ashes, by the fluorine of which I have only one etching to shew. It is not a favourable specimen; the ashes were washed with a considerable volume of hydrochloric acid and water; the product of distillation was tested by the less perfect potassium-process; and the lines etched by the hydrofluoric acid were drawn too fine. Experience has taught my assistants that the wax should be spread thin, and the lines through it be made with a broad point, if a distinct etching is to be obtained. But, withal, the results with coal-ashes are sufficiently marked.

I have further tested the sufficiency of the ammonia process in the following stringent way. A fossil bone from the Himálayas, which I had already ascertained to contain a fluoride, and which was full of crystals of carbonate of lime,

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was reduced to powder, and mixed with powdered glass so as to add to it excess of silica. It was then subjected to the ammonia process, and has yielded an etching as deep as the purest fluorspar could have given with oil of vitriol.

The result is so marked, that I should recommend the deliberate addition of silica to bodies suspected to contain fluorine, as a provision for permitting such substances to be analysed in *glass* vessels, in which the largest quantities may be subjected to examination without risk of missing the element in search, or permitting it to escape.

Five points call for further notice.

1*st*, When a silicated fluoride, as I may, for the sake of brevity, call it, is distilled with oil of vitriol, the whole of the fluoride of silicon comes away as gas, as soon as the oil of vitriol has reached its boiling-point. It is not necessary, accordingly, to subject a body supposed to contain fluorine to any lengthened ebullition ; and, in the case of plant-ashes, it is desirable to arrest the boiling as soon as all the fluorine has been evolved, for protracted ebullition only occasions evolution of sulphurous acid. Besides the ultimate glass-etching, the escape of fluorine is rendered manifest by the appearance of a white gelatinous body in the water, through which the gas evolved ($Si F_3$) is passed ; and by the production of a gelatinous, flocculent precipitate, when the solution of this gas is neutralised with potass. The coal-ashes gave all those results.

2*d*, It appears exceedingly probable, that much of the silica occurring in the forms of quartz, chalcedony, opal, sinter and the like, which is generally supposed to have been deposited from aqueous or alkaline solution, has owed its origin to the decomposition of fluoride of silicon by water, or has otherwise been related to fluorine as its solvent or transferring agent. This, or rather the less precise notion of fluorine conveying silica, has been suggested by my friend Mr A. Bryson, and by Dr H. Buchanan, E.I.C.S.

3*d*, The occurrence of fluorspar in drusy cavities in greenstone, along with silica, as in the specimens obtained from Bishopton, on the Clyde ; the similar occurrence of apophyllite in the cavities of trap ; the association of topaz, pycnite,

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lepidolite, and most of the other compound fluorides, with granite, gneiss, and mica slate, will acquire additional significance from the discovery that fluorine occurs in the rocks which form their matrices.

4th, The presence of fluorine in plants is now rendered doubly probable, as it may enter them alike in combination with a metal such as potassium, sodium, or calcium, or in association with silica.

5th, The presence of fluorine in animals may now be fully accounted for; as it not only enters their bodies in the water they drink, but is contained in the vegetable food, by which, directly or indirectly, the whole animal kingdom is sustained. The prosecution of these views, however, will be taken up in succeeding papers.

On the Presence of Fluorine in the Stems of Gramineæ, Equisetaceæ, and other Plants; with some Observations on the Sources from which Vegetables derive this element. By GEORGE WILSON, M.D.*

Table of Plants examined for Fluorine. The numbers represent grains of ashes, except in the case of Tabasheer and Wood Opal. The blanks imply that the weight was not known.

Ashes, in Grains.	Name of Plant.	
200	Horsetail (Equisetum limosum), . .	Distinct etching.
	Common bamboo (Bambusa arundinacea), .	...
	Charcoal (derived chiefly from oak, and to a smaller extent from birch), . . .	...
	Coal, ...	...
	Barley straw, ...	...
	Hay (Ryegrass), ...	...
35	Equisetum variegatum, ...	Faint etching.
19	——— hyemale, ...	...
255	——— palustre, ...	...
	Tussac grass (Dactylis cæspitosa), . .	...
99	Elymus arenarius, ...	...
495	Sugar cane (Saccharum officinarum), . .	...
1040	African teak, ...	...
	Smilax latifolia, ...	No etching.
	Common rosemary (Rosmarinus officinalis), .	...
235	Nepaul bamboo (Bambusa Nepalensis), . .	...
	Common Fern (Polypodium vulgare), . .	...

* Read to the Botanical Society of Edinburgh, July 1852.

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537	Tree Fern, ...	No etching.
24	Phalaris arundinacea, ...	...
240	Malacca cane, ...	...
50	Cocoa-nut shell, ...	...
127	Indian teak (Tectona grandis), . . .	...
80	Tabasheer, ...	...
1680	Wood opal, ...	...

On this table the author remarked, that the siliceous stems which he had found to abound most in fluorine, were exactly those which contained most silica. In particular, deep etchings were procured from the Equisetaceæ (horsetails), and from the Gramineæ (grasses), especially the common bamboo. The last was known to contain silica in such abundance that it collected within the joints in white masses, nearly pure, and had long, under the name of Tabasheer, been an object of interest to natural philosophers. The horsetails were scarcely less remarkable, for the amount of silica contained in their stems, which had led to the employment of one of them (Dutch rush, Equisetum hyemale), in polishing wood and metals. The African teak, which, like the bamboo, is known sometimes to secrete silica, was also found to contain fluorine, though much less largely than the plants named; whilst the strongly siliceous stems of barley and ryegrass also yielded the element in marked quantity. The sugar-cane, however, gave less striking results than might have been expected, and the same remark applied to the Malacca cane. Two specimens of silicified wood and one of Tabasheer gave no evidence of the presence of fluorine. So far, however, as the plants named in the preceding Table, are concerned, the author does not wish it to be inferred, from the negative results which are detailed, that the plants in question are totally devoid of fluorine. With larger quantities of their ashes, positive results would in all probability be obtained.

The author's general conclusions were as follow:—1*st*, That fluorine occurs in a large number of plants; 2*d*, That it occurs in marked quantity in the siliceous stems of the *Gramineæ* and *Equisetaceæ*; 3*d*, That the quantity present is in all cases very small, for although exact quantitative results were not obtained, it is well known that a fraction of a grain of a fluoride will yield, with oil of vitriol, a quantity of hydrofluoric acid sufficient to etch glass deeply, so that the proportion of fluorine present, even in the plant ashes which contain it most abundantly, does not probably amount to more than a fraction per cent. of their weight. The proportion of fluorine appears to be variable, for different specimens of the same plant did not yield concordant results.

In this, however, there is nothing anomalous, for some Bamboos yield *Tabasheer* largely, whilst others are found to contain none. It seems not unlikely that soluble fluorides ascending the siliceous stem of a plant, on their way to the seeds or fruits in which they

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finally accumulate, may be arrested by the silica, and converted into insoluble fluosilicates (fluorides of silicon and of a metal); and a Bamboo, for example, secreting Tabasheer, may effect this change where one less rich in silica cannot determine it. The slow or quick drying of a stem may also affect the fixation of fluorides in the stems or trunks of plants.

The sources of the fluorine found in plants may be regarded as pre-eminently two,—(1.) Simple fluorides, such as that of calcium, which are soluble in water, and through this medium are carried into the tissues of plants; and (2.) Compounds of fluorides with other salts, of which the most important is probably the combination of phosphate of lime with fluoride of calcium. This occurs in the mineral kingdom in apatite and phosphorite, and in the animal kingdom in bones, shells, and corals, as well as in blood, milk, and other fluids.

The recent discovery of the author's communicated to the Royal Society of Edinburgh, (page 349) has shewn that fluorides are much more widely distributed than is generally imagined, and that the trap rocks near Edinburgh, and in the neighbourhood of the Clyde, as well as the granites of Aberdeenshire, and the ashes of coal contain fluorides, so that the soils resulting from the disintegration of those rocks cannot fail to possess fluorides also. All plants, accordingly, may be expected to exhibit evidences of their presence, in the following portions of their tissues or fluids :—

1. In the ascending sap, simple fluorides.
2. In the descending sap, in association with the albuminous vegetable principles, and in the seeds or fruits, in a similar state of association, fluorides along with phosphates.
3. In the stems, especially when siliceous and hardened, fluorides in combination with silica. The investigation is still in progress.

Observations on the Relation between the Height of Waves and their Distance from the Windward Shore; in a Letter to Professor Jameson. By THOMAS STEVENSON, Esq., F.R.S.E., Civil Engineer.

EDINBURGH, *September* 16, 1852.

DEAR SIR,—In designing a harbour or sea work, the engineer, in order to avail himself of the advantage which is to be derived from past experience, must endeavour, to the best of his power, to institute a comparison between the given locality and some other which he supposes to be *in pari casu*. Such a similarity, however, of locality and other physical peculiarities is hardly, if ever attainable, and all that he can

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do in such circumstances is to select an existing work which is as nearly as possible similarly exposed. Perhaps the most important element in such a comparison is what may be termed the *line of maximum exposure*, or in other words, the line of greatest "fetch" or reach of open sea. This line can be measured from a chart, and in this manner the difference of exposure at the existing harbour and at the place where the new one is to be built, may be ascertained, but the engineer still does not know in what ratio the height of the waves increases in relation to any given increase in the line of maximum exposure.

As this inquiry is one of great importance in the practice of marine engineering, and has not, so far as I know, been in any way investigated, I have, during the last two years, been making occasional observations on the subject when favourable circumstances occurred, and when my professional avocations would permit me. The localities selected were a small fresh-water loch, the Frith of Forth, and the Moray Frith.

These observations have been but limited in extent, and I have thought it proper therefore to avail myself of your far-spread Journal, in directing the attention of others to the prosecution of this inquiry, which can be perfected only by multiplied trials. So far as my own observations have as yet gone, the waves seem to increase in height most nearly *in the ratio of the square root of their distances from the windward shore*.—I remain, yours faithfully,

THOMAS STEVENSON.

Professor JAMESON.

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Additional Observations on the Green Teas of Commerce.

By Robert Warrington, Esq., F.C.

Since the publication of my last communication on this subject, read before the Society, May 19, 1851,* a series of microscopical and chemical examinations have been published in the *Lancet* of 9th August 1851, which have induced me to institute some additional experiments, the results of which may not be without interest to our readers, particularly, as they tend to remove a curious anomaly that has lately arisen. In the series of examinations alluded to, it is stated that several of the specimens of the green tea submitted to investigation, were coloured with indigo mixed with porcelain clay; and this is followed by an examination of some of the colouring materials themselves used at Canton for this purpose, and which had been obtained from the Museum at Kew Gardens. As I had stated† that, up to that period, no sample in which indigo had been employed as an artificial colouring agent for green teas had come under my notice, I felt it incumbent on me to investigate the matter. For this purpose I applied to Sir W. Hooker on the subject, and he allowed me in the handsomest manner to take from the cases in the Museum, small portions of the materials for examination, and also favoured me with the loan of the manuscript journal of Mr Berthold Seeman, by whom the specimens had been collected while at Canton, as naturalist of H. M. Ship 'Herald,' then on a survey in that quarter of the globe. As these documents have been since published, and as the subject opens some interesting particulars, I have taken the liberty of appending his account in his own words.‡

* This Communication is transfered to our Journal, vol. li., p. 240. [*Ed. Edin. Phil. Journ.*]

† Quart. Jour. Chem. Soc. iv., p. 156.

‡ Hooker's Journal of Botany, and Kew Garden Miscellany, No. 37, for January 1852. "In the Manual of Scientific Inquiry, you ask, whether, in the northern provinces of China, indigo or any other vegetable dye is used in colouring green tea? Whether different processes of dying are pursued in the north from those of the south I cannot say, but it is certain that around Canton, whence great quantities are annually exported, the green tea is dyed with

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Mr Seeman here distinctly states, that around Canton the *green tea* is dyed with *Prussian blue*, *turmeric*, and *gypsum*; that in the manufacture he inspected, the *dyes above mentioned* were added; and he gives their proportions. That there was *no concealment* or mysterious proceeding; that one of the great merchants conducted him over his own, and also another manufactory, and that everything was conducted openly, and exhibited with great civility. And yet, strange to say, Mr Seeman appears to have been deceived notwithstanding all this; for on submitting these materials to the

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Prussian blue, turmeric, and gypsum, all reduced into fine powder. The process is well described by Sir J. F. Davis ('The Chinese,' iii., 244), who, however, falls into the strange mistake of supposing the whole proceeding of colouring to be an adulteration, and leaves his readers to infer that it is only occasionally done in order to meet the emergency of the demand, while it is now very well known that all the green tea of Canton has assumed that colour by artificial dying. I had heard so much about tea, copper plates, picking of the leaves, rolling them up with the fingers, boiling them in hot water, &c., that I became anxious to see with my own eyes the process of manufacture, of which the various books had given me such a confused idea. One of the great merchants conducted me not only to his own but also to another establishment, where the preparation of the different sorts was going forward. There was no concealment or mysterious proceeding, every thing was conducted openly, and exhibited with the greatest civility; indeed, from all I saw in the country, I am almost inclined to conclude that either the Chinese have greatly altered, or their wish to conceal and mystify everything, of which so much has been said, never existed.

"The tea is brought to Canton unprepared; after its arrival it is first subjected to cleaning. Women and children are employed to pick out the pieces of twigs, seeds, and other impurities with which it happens to be intermixed. The only sorts which may be called natural are those gathered at different seasons; the rest are prepared by artificial means.

"Without entering into a description of all these processes, it may suffice to take one as an example. A quantity of Bohea Saushung was thrown into a spherical iron pan kept hot by means of a fire beneath. These leaves were constantly stirred about until they became thoroughly heated, when the dyes above mentioned were added, viz., to about twenty pounds of tea, one spoonful of gypsum, one of turmeric, and two or even three of Prussian blue. The leaves instantly changed into a bluish green, and having been stirred for a few minutes, were taken out. They, of course, had shrivelled and assumed different shapes from the heat. The different kinds were produced by sifting. The small longish leaves fell through the first sieve and formed young Hyson, while those which had a roundish granular shape fell through last, and constituted Choocha or Gunpowder."

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action of chemical tests, there could be no doubt that they consisted of indigo of a very inferior quality, and leaving a very large proportion of inorganic matter by calcination, and of porcelain clay. It is also curious that the very case selected by Mr Seeman to illustrate the processes, is the conversion by means of this facing or glaze, of a low quality of black tea (Bohea Saushung) valued at about 4d. to 6d. the pound, into high quality green teas valued at from 1s. to 1s. 6d. the pound; but although Mr Seeman does not allow this to be an adulteration, yet surely he cannot deny that it is a fraud.

Another very good method which I have lately employed of removing the colouring matter from the surface of green teas for the purpose of microscopical investigation, and one attended with very little trouble, is to take a piece of cream coloured wove paper, or paper free from blue colouring material, and having breathed on its surface or rendered it slightly damp, to pour the sample of tea under examination from the containing paper or vessel upon it. On then removing it back again a quantity of the facing powder will be found adhering to the surface of the paper; and on placing it under the microscope it will be found studded with the colouring materials used, and the blue particles can be subjected to the action of chemical tests with the greatest ease, by placing a minute drop of the reagent on the granules with the end of a small stirring rod or slip of glass, and noting the effect.—(*The Quarterly Journal of the Chemical Society*, Vol. v., No. 18, July 1852, p. 139.)

On the Distribution of Granite Blocks from Ben Cruachan. By WILLIAM HOPKINS, Esq., F.R.S., President of the Geological Society.

Mr W. Hopkins exhibited at the Meeting of the British Association at Ipswich, in 1851, a map of the lochs and mountains around Ben Cruachan, with the distribution of the trains of granite blocks, to which he had alluded last year at the Edinburgh meeting. He had for-

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merly been unable to explain by what means the granite blocks, supposed to have been derived from Ben Cruachan, had crossed the mountain group between Loch Fyne and Loch Lomond, so as to gain access to the latter, and form a stream extending to the Clyde and Glasgow. Since then, he had discovered, in this very mountain group, a granitic tract, not marked on the geological maps, in the immediate vicinity of Loch Sloy, at a height from 1500 to 2000 feet, and agreeing in mineral character with these travelled blocks, which may therefore have descended Loch Long and Loch Lomond, with the same facility that the granite blocks of Ben Cruachan have entered Loch Awe, and those of Loch Etive have reached Oban and Kerrara. They are dispersed along the sides of the valleys, to the height of 300 or 400 feet. Mr Hopkins then referred to the possible causes of the dispersion of the granite blocks;—if by ocean currents, then the country must have been depressed nearly 2000 feet, as Wales is believed to have been about the same period; if transported by floating ice, independently of glaciers, then also the country must have had a lower level: terrestrial glaciers may also have been agents, if their existence was allowed. The character of the blocks,—being at first large and angular, but becoming smaller and more rounded,—was opposed to the supposition that floating ice or terrestrial glaciers were the principal agents in their removal. If floating ice had been the cause, then the sphere of dispersion would probably, also, have been much greater. In Glen Wray he had observed indications of what he had considered true moraines. He was inclined to believe that more than one of these causes had been in operation in the dispersion of these blocks from their respective centres.

On Fish Destroyed by Sulphuretted Hydrogen in the Bay of Callao. By Dr J. L. BURTT, U.S.N. (Proc. Acad. Nat. Sci. Philad., vi. 1.)

One occurrence always excited much interest, whenever there was an evolution of sulphohydric acid gas (a frequent occurrence) from the bottom of the bay of Callao. The first

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premonition of what was to produce a remarkable destruction among fish, was the discoloration of the water of the bay, from a marine green to a dirty milk-white hue, followed by a decided odour of the gas; so much of it being present on many occasions as directly to blacken a clean piece of silver, and to blacken paint-work in a few hours.

The fish, during this evolution, rose in vast numbers from the bottom, and after struggling for some time in convulsions upon the surface, died.

I was particularly struck by this fact, that all of them, during the time they were under its influence, acted in precisely the same manner. The first thing noticeable with regard to its effect upon them was, that on coming near the surface, they seemed to have much difficulty in remaining below it at all. They then rose completely to the surface, struggling vainly to dive beneath. This was followed by a violent springing and darting in various directions,—evidently without control of direction—for they moved sideways, or upon the back, and sometimes tail first, with great velocity. After a little time their motion became circular, and upon the back, the circle of gyration constantly diminishing, and the rapidity of the motion as constantly increasing, until there was a sudden cessation of all motion. The head then floated above the surface, the body being in a perpendicular position. A few convulsive movements shortly followed, and they were dead.

I have watched thousands of them so dying, and in every instance such was the mode of death. Having taken them at the moment of death, and immediately after, a rude examination shewed in all the same appearance. The intestines and brain were gorged with blood, much darker than natural. The gills were almost black, and the air-bladder ruptured.

M. Melloni on Dew.

Dew is not an immediate effect of the cooling produced by the nocturnal radiation of vegetables on the vapour of the atmosphere, as most treatises on physics and meteorology

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assume, but the result of a series of actions and reactions between the cold due to the radiation of plants, and the cold transmitted to the surrounding air. The grass is cooled but little below the temperature of the air, but it very quickly communicates to it a portion of the acquired cold; and since the difference of temperature between the radiating body and the surrounding medium is independent of the absolute value of the prevailing temperature, the grass surrounded by a colder air still further lowers its temperature, and communicates a new degree of cold to the air, which reacts, in its turn, on the grass, and compels it to acquire a temperature still lower, and so on in succession. Meanwhile the medium loses its state of equilibrium, and acquires a sort of vertical circulation, in consequence of the descending motion of the portions condensed by the cold of the upper foliage, and the ascending motion of the portions which have touched the surface of the earth. Now, the gradual cooling and the contact of the soil evidently tend to augment the humidity of the stratum of air, and thus bring it by degrees towards the point of saturation. Then the feeble degree of cold produced directly by the radiation of bodies, suffices to condense the vapour contained in the air which surrounds them; and since the causes which give rise to the circulating movement, and to the humidity of the air, continue through the whole of the night, the quantity of water deposited on the leaves increases indefinitely.

The greatest part of the nocturnal cooling is due to the development of the leaves, which presents to the sky an immense number of thin bodies having large surfaces, and almost completely isolated; this is the reason why the dews are so feeble in winter, and less copious in the nights of the early parts of spring, than in the equally long nights of autumn. Dew is also more abundant in autumn, because the days being then warmer than in spring, and the vapour increasing more rapidly than the temperature, the same degree of cold (such as the invariable depression of the temperature of plants below that of the atmosphere) condenses a greater quantity of vapour. The slightest breath of wind disturbs the circulation of the lower atmospheric stratum, and neces-

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sarily diminishes the accumulation of dew. A strong wind impedes its formation, by bringing fresh supplies of heat, and especially by renewing incessantly the stratum of air comprised between the summit of the plants and the surface of the earth, and thus taking away from it the possibility of gradually acquiring that high degree of humidity necessary to the precipitation of the vapour, by reason of the small degree of cold which the plants contract with regard to the surrounding medium.

The differences of the quantity of dew on different substances all arise, either from their difference of emissive power, or from the diversity of their situation with regard to the heavenly vault, or from the hygrometric condition of the surrounding space, or from the greater or less obstacles which retard the descent of the air, and thus more or less favour its frigorific reaction; or, lastly, from the proximity of the soil, which permits the return of the air on the radiating substances, and gives rise to that aërial circulation, whence result the gradual cooling and successive augmentation of humidity in the lower stratum of the atmosphere.

Distribution of Dew in different Regions.

To complete the study of our subject, it now only remains for us to examine the intensity of the nocturnal radiation, and the distribution of dew in the different regions of the globe.

Many observations have been made to determine the diurnal temperature in different parts of the world, but very few with the object of determining the nocturnal heat; so that we are almost entirely ignorant as to what are the true proportions between the temperatures of day and night in different latitudes and seasons of the year. In accordance, however, with the preceding remarks, it is seen that in calm and clear seasons, the difference between the temperature of the day and of the night ought to be so much the greater, as the vegetation is richer and the night longer; and we have already observed, that in the nights of the early part of spring, vegetation being but little developed, the temperature is less lowered than in the latter part of autumn, when the plants still preserve a part of their foliage. We

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shall now add, that in these countries where the foliage is generally narrow and vertical, like that of New Holland, the nocturnal temperature ought to be less diminished, relative to the diurnal temperature, than in places of the same latitude covered with plants analogous to those which grow in ether countries.

Copiousness of Dew in Tropical Countries.

But, laying aside everything depending on the alternations of the seasons in our temperate climates, and on the differences of vegetation in countries situated under the same latitude, it is easy to convince ourselves, that the greatest difference between the temperature of the day and that of the night will occur under the torrid zone, and that there also the dews will, in general, be more abundant than in any other part of the globe. In fact, in cold and temperate countries, the two principal elements of nocturnal radiation proceed (so to speak) in opposite directions; since the night is long when the earth is destitute of vegetation, and short when the plants are richly clothed with foliage. But under the equator, vegetation never fails, the night is always long, and almost entirely without twilight; and in the neighbouring countries forming the torrid zone, properly so called, when the night time slightly exceeds the period of daylight, the rain falls in torrents, and plants are more richly clothed with leaves than at any other season of the year. The greatest difference, then, between the temperature of the days and that of calm and clear nights will occur in the equatorial regions, a short time after the rainy season; and as there will then prevail in the atmosphere a high degree of humidity, the dew itself also will be very abundant at this season. On the other hand, since the torrid zone possesses the highest known atmospheric temperature, the nocturnal cooling ought to precipitate there a larger quantity of water than in any other country, by reason of the divergence above mentioned between the progression of the vapour and that of the temperature. In fact, the dews are so copious in the equatorial regions, that M. de Humboldt does not hesitate to compare their effect with those of rain itself.

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Want of Dew in Polynesia.

A curious fact, and one not much known, which seems at first sight to contradict what we have been saying, is the extreme feebleness, or the absolute non-existence of dew in that extensive assemblage of small islands in the torrid zone, generally fertile, and more or less rich in plants, which geographers denominate Polynesia.

But, with a little attention, it will soon be seen that this apparent anomaly affords one of the most striking confirmations of the truth of the theoretical views unfolded in the course of this memoir. In fact, whatever may be the humidity of these small islands, scattered here and there in the vast ocean like oases in a desert, and their tendency to the cooling produced by the long nights and luxuriant vegetation, the small extent of their territories renders the atmospheric column superincumbent on each of them easily permeable even to its centre by the air of the surrounding sea. This invasion is, moreover, favoured by the trade-winds which prevail constantly in those latitudes. Now we know that the air in the midst of vast seas preserves a nearly uniform temperature. The stratum of air cooled by the contact of the soil will then be warmed by mixing with the air which is constantly reaching it from the sea, and the difference between the temperatures of the day and night being extremely small, dew can scarcely be formed at all, or, at any rate, in very slight quantity.

Want of Dew on Ships traversing the vast solitudes of the Ocean.

Perfectly analogous causes prevent the formation of dew on ships which traverse the vast solitudes of the ocean. But, what is truly singular, is the appearance of the phenomenon on board these same ships on arriving afterwards in the neighbourhood of *terra firma*. Thus the navigators who proceed from the Straits of Sunda to the Coromandel Coast, know that they are near the end of their voyage when they perceive the ropes, sails, and other objects, placed on the deck, become moistened with dew during the night (Le Gentil,

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Voyages, tome i., page 625.) The reason of this strange phenomenon will readily be seen, if we start from the fact (well established by experience), that, in the equatorial regions, the sea air preserves not only a nearly constant temperature by day and night, but also an hygrometric state considerably removed from the point of saturation ; and that the reverse is the case with regard to the air on land, which, in the day time, is drier than the air of the sea, but which, in the night, may readily acquire, in countries sufficiently abounding in water, or near enough to the coast, a much greater humidity, in consequence of the frigorific actions and reactions of which we have before spoken. Now, the land wind, which always blows by night on the borders of tropical countries, when the sky is clear, transports this humid air to a certain distance out at sea. Then, the feeble degree of cold acquired by substances freely exposed on the deck, totally unable, as it is, to condense the vapour of the sea atmosphere, is nevertheless sufficient to precipitate that of the air which has been in nocturnal contact with the soil.

Dew becomes more abundant as we approach the Equator.

We conclude that dew, feeble or non-existent towards the poles, by reason of the extreme brevity of the summer nights, becomes more and more abundant as we approach the equator ; that, notwithstanding the general course of the phenomenon is very much modified by the extent, the nature, and the position of the land, according as it is more or less surrounded by the sea, more or less covered by mountains, ravines, lakes, meadows, marshes, or running streams. The borders of Egypt, of the Red Sea, of the Persian Gulf, of Chili, and of Bengal, are celebrated for the richness of their dews (see the *Voyages de Volney*, t. i., p. 51 ; *of Burckhardt*, p. 423 ; of *Niebuhr*, p. 10 ; of *Ker Porter*, t. ii., p. 123 ; of *Le Gentil*, t. i., p. 624 ; of *Ruppel*, p. 186) ; the deserts of Central Africa, and the interior provinces of Bahia ; of Fernambouc, Urmia, and Mazandecan, in Brazil and Persia, by the almost total absence of this nocturnal phenomenon. (*Voyages of Spix* and *Martius*, t. ii., p. 624 ; of *Oliver in Persia*, t. i., pp. 123, 145 ; of *Ker Porter*, t. ii., pp. 63, 69.)

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Presence of Dew makes known the proximity of Masses of Water concealed from the Eye.

The appearance of dew may serve, in certain cases, to make known the proximity of a mass of water concealed from the eye of the observer. Thus the dew, which is almost completely wanting in certain steril valleys traversed by the Euphrates, becomes of sufficient intensity to form visible drops of water, whilst at a distance of some miles from the borders of this river, concealed by the land (Oliver., t. ii., p. 225). And Major Denham says, that independently of the suffocating heat, and of the intense cold that he endured during the night in his memorable journey across the Sahara, he also suffered from the extreme dryness of the air, until he reached a certain distance from Ischad, where, though there was not the slightest appearance of water on any part of the horizon, the dews began to appear feeble at first, then more and more copious, and so abundant on arriving near the banks of this great African lake, that the clothes of those persons who remained sometime outside the tents were completely soaked with it.—(Denham, *Narrative*, p. 49.)

Intense Cold during the Night in the Great Desert.

With regard to the intense cold experienced by this intrepid traveller, Denham, during the night in the Desert, it is occasioned (in my opinion), neither by the extreme clearness of the sky, nor by an excess of cutaneous perspiration, but from the great nocturnal calm of this desolate region, which allows the soil to act strongly on the air, and to receive with equal force the reaction of that fluid. Observe, first, that a dry, flat, monotonous, horizontal, and uniformly extended country, like this immense plain of Northern Africa, so well characterised by the Arabs under the name of the *Sea without water* (*El baar billa mâa*), presents no cause capable of disturbing, during the night, the equilibrium of the air; so that this must remain in a state of almost absolute rest some time after the setting of the sun. The soil of the Desert being, moreover, composed of dry, sandy earths, of bad conducting quality, can receive from the interior but a very poor com-

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pensation in exchange for the heat it has lost. The solid body radiating by night towards space, and the surrounding medium, will therefore be unmoving and isolated, and thus be in highly favourable conditions for reacting with energy on each other, and considerably lowering their temperature.

Artificial Freezing of Water in Bengal.

Another phenomenon resulting from the combination of the two frigorific actions, successively excited in the radiating body, and the medium which envelopes it, is the congelation of water, produced artificially in Bengal, during the calm and clear nights. It would be superfluous to repeat here the details relative to this process, a description of which may be found in all treatises on physics. It will be sufficient to call to mind, that the vessels, very shallow and uncovered, containing the liquid to be frozen, are placed at the bottom of certain excavations made in the soil, and surrounded by a border of earth, four or five inches in height; that the water, whose emissive power is nearly equal to that of the leaves of plants, and of lamp black, does not descend even two degrees lower than a covered thermometer placed by its side, and that frequently the ice is formed when the thermometer, elevated four or five feet, marks 5° or 6° above zero; which leads to the immediate inference, that the water lowers gradually its temperature down to the zero of the thermometric scale (centigrade), by means of a series of actions and reactions, perfectly similar to those which produce, under the same circumstances of calm and clearness of sky, the nocturnal cooling of any other radiating matter exposed to the free air, and the decrease of the atmospheric-temperature, in proportion as we approach the earth's surface.

It is in consequence of these same frigorific actions that the buds of plants, and the shallow waters of ditches and ponds, scattered here and there over the country, often freeze during the calm and clear nights of spring, whilst the thermometer marks several degrees higher than the freezing point.—(*Extracted from Melloni's Memoir on Dew.—Richard Taylor's, Esq., F.S.A. Sc., Scientific Memoirs*, Vol. v., Part xx. April 1852, p. 543.)

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Obituary.

We regret to announce the death of a learned and highly accomplished naturalist, William Macgillvray, Professor of Civil and Natural History, Marischal College, Aberdeen. Dr Macgillvray, originally, we believe, from the Island of Harris, was for many years assistant to Professor Jameson, in the University of Edinburgh. He was afterwards appointed Conservator of the Anatomical and Surgical Museum of the Royal College of Surgeons, Edinburgh. This office he resigned on being appointed to the Chair of Civil History and Natural History in Marischal College, Aberdeen. He lectured for many years with great success to enthusiastic classes of students, and increased the interest and utility of his excellent academic prelections by field lectures and excursions. His extensive acquaintance with all the branches of natural history, and his eminent talent as a writer, occasioned great demands on his time; and it is well known that considerable and important works connected with natural history owe their chief value and charm to his learning and genius.

Ornithology, botany, and geology, were with him favourite pursuits. His great and beautiful work, entitled "A History of British Birds, Indigenous and Migratory," &c., in five volumes octavo, with numerous characteristic engraved illustrations from his own beautiful drawings, is universally known and esteemed. He translated several volumes on Botany; but published no original work in that department of Natural History. In Geology, however, he contributed interesting memoirs to scientific societies, and to scientific journals of the day; and published a manual of that popular science, which, although incomplete, is on a better plan than that adopted in some similar works of greater pretensions.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Meteorological Society at the Mauritius.*—It is pleasing to learn that a Meteorological Society has been formed last summer at

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the Mauritius under the auspices of the Government, which, from the names of its Councillors, and the very good regulations which it has issued, promises to obtain much novel information from that colony, and the surrounding ocean.

2. *Great Fall of Rain in India.*—Professor Oldham, in writing to Sir R. I. Murchison from Churra Poonjee, in the Khassya Hills, north of Calcutta, states that the rain-fall is there about 600 inches, or 8½ fathoms, per annum; 550 inches of which descend in the six rainy months commencing in May; and that in one day he measured a fall of 25·5 inches.

3. *Annual Amount of Rain at Alexandria.*—The annual amount of rain at Alexandria stands in contrast to that mentioned as occurring in some places in India; the quantity at the former being only 7½ inches. This quantity, indeed, might be expected to be small, from our knowledge of the fact that three or four degrees to the south the country is nearly rainless.

GEOLOGY.

4. *Examination of Rocks by means of the Microscope.*—Many years ago, we strongly recommended the use of the microscope in examining the structure of rocks, especially of quartz rock and sandstone; also of compact rocks, as basalt and clinkstone. Very lately this important subject has engaged the attention of Naturalists, as is shewn by the circumstance that, at the meeting of the British Association in the year 1851, at Ipswich, a memoir was read on Klinology; and that, at the meeting of the British Association in the present year at Belfast, the examination of rocks by means of the microscope was explained and illustrated in a very interesting manner by several of the more distinguished members of the Association. We trust that ere long the results of these examinations, which so deeply excited the curiosity and attention of the meetings, will be laid before the public.

5. *On the Relative Conducting Power of Rocks for Heat.*—M. G. D. Helmersen, in a set of experiments on the relative conducting power of some rocks for heat, finds that quartz rock is the best conductor of heat, and compact limestone the worst.

6. *Tertiary Coal in India.*—In the Sylhit district in Bengal there is a deposit of tertiary nummilitic limestone connected with a deposit of coal and ironstone. The coal is called by the reporter *true coal*. What is meant by true coal? Geologists enumerate three sets of coal, viz., anthracite or glance coal, black coal, and brown coal. To which of these are we to refer the *true coal*?

7. *Examination of Soils by the Microscope.*—The microscopic examination, by Ehrenberg, of the black earth or soil (Schwarzerde)

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of Tachernosem in Russia, remarkable for its fertility, and which covers 60,000 geographical square miles of country to a depth from half a yard to two yards and a half, is a fine example of the utility of microscopic examination of soils. This black earth was proved by this examination to be a fresh-water deposit, and that probably its extraordinary fertility was in some degree connected with its abundance of microscopic fossil animals and plants, and their remains.

8. *Rock Salt of the Punjaub in India.*—Dr Andrew Fleming, in the medical service of the Hon. East India Company, has ascertained the geological position of the salt in the North Punjaub to be below the carboniferous limestone, in the form of a bed or beds. Geologists consider this geological discovery as one of great importance.

9. M. Elie de Beaumont, in his first memoir, read to the French Academy in June 1829, on Mountain Systems in Europe, indicated four systems; soon after he indicated nine, then twelve, and laterally twenty-one. He now considers it probable that before long, if the study of this department of geology is continued, that the number of systems will be above a hundred.

10. *Survey of the suppositious Submarine Bridge of the Norwegians.*—The survey of the so-called long "sea-bridge" (Havbroe), which was supposed to range along the coast of Norway, is finished, and shews that the Jutland bank stretches west and north to about 61°, but is separated from the Norwegian bank by a channel nearly 200 fathoms deep; that the fishing grounds between Stal and Christiansand are not so distant from the coast as was supposed, and are completely separated from the Jutland bank; and hence the tradition of the existence of a continuous submarine bridge between the coast of Norway and the Continent is a fable. These banks prove to be, in fact, as every geologist would *à priori* suppose, the representations, under the sea, of the detached "Osar" of the Swedes, and the Skyærgaarden of the Norwegians, as seen in the water-worn gravel ridges of the present continent of Scandinavia.—(*Address at the Anniversary Meeting of the Royal Geographical Society, 24th May* 1852. *By Sir R. I. Murchison*, p. 42.)

11. *On the Pterodactyles of the Chalk Formation.*—Mr Bowerbank, at the meeting of the British Association at Ipswich, in 1851, exhibited drawings and restorations of remains of these winged reptiles, shewing that the great species of the chalk (*P. Cuvieri*) must have had a spread of wing equal to 16 feet 6 inches; whilst a second large species (*R. compressorostris*) was estimated at 15 feet. The largest species from the lias, previously well known, the *P. macronix* of Buckland, was only computed at 4 feet 7 inches from tip to tip of its expanded wings.

12. *On the Remains of a Gigantic Bird from the London Clay*

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of Sheppey. By J. S. Bowerbank, F.R.S.—The specimen described is a fragment of one of the bones of the extremities. It is 4 inches long, and 1 inch in diameter at the longer end, and is somewhat three-sided with rounded angles. The thickness of its walls is from $\frac{3}{4}$ of a line to $1\frac{3}{4}$ line; its microscopic structure exhibits the characteristic bone-cells of animals of the bird tribe. The specimen indicates the bird to have been at least the size of a full-grown albatross. —(*British Association Report for* 1851.)

13. *Map of Switzerland.*—In speaking of the progress which has been made in the topographical survey of Switzerland, I would specially direct your attention to four sheets of the cantons of Appenzell and St Gallen, which M. Ziegler of Winterthur, who has drawn and executed them, has just presented to us. They form part of a survey on the same large scale of $2\frac{1}{2}$ inches to a mile, or $\frac{1}{25,000}$, which is also in the course of application to the cantons of Zurich and Schaffhausen. To give full effect to these four sheets only, M. Ziegler passed six consecutive summers in the mountains and valleys of St Gallen and Appenzell, the geometrical measurements of which had been made under the direction of M. Eschmann. The inspection of the results will, I am sure, lead any of you who have studied map-making to agree with me, that they are examples of a fidelity to nature which has rarely been attained. M. Ziegler soon found (M. Leopold von Buch and M. Escher von der Linth being his counsellors) that every class of rock has a peculiar "facies," and hence he became convinced, that no really good topography can be made by surveyors who neglect geological data. Thus, in these sheets, the eye of a geologist at once seizes the rugged escarpment of slaty rocks, the undulations of limestone, or the bosses of conglomerate or nagelflue; whilst, from personal inspection of a portion of the difficult region here represented, I can truly say, that I never yet saw a map more completely ready to receive the colours of a field geologist. The lights are all thrown in perpendicularly, so that the defects of the maps of Geneva and Vaud, as proceeding from oblique shading, are avoided, and the altitude of each terrace, valley, or mountain top, is inserted in numbers on a most exquisitely finished lithographic relief. I am authorised by M. Ziegler to say, that, if the large scale of $2\frac{8}{10}$ inches to a mile had not been determined upon, he could have delineated as effectually all the same features on a scale of about $1\frac{1}{2}$ inch to a mile. In these works we perceive at a glance the value of good hill-shading; and when the map of the magnificent mountain of Sentis, which stands out to the low countries of Germany as the great sentinel of the Swiss Alps, is forwarded to us, you will see in it how perfectly such a work may supersede the want of any model whatever.

The largest part of the cantons of the Grisons and of Tessin has been surveyed; but detailed maps of this mountainous region are still wanting, as well as those of large portions of Berne, which are

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constructing on the scale of the general Swiss map directed by General Dufour, or $\frac{6.4}{1\,0\,0}$ inch to the mile. It is much to be regretted that the scale of these Swiss maps varies in different cantons. In the meantime, we are much indebted to M. Ziegler for a small, useful, general map of Switzerland, which he has published, and which will, I am assured, be soon coloured geologically by Professor Studer of Berne, whose acquaintance with the structure of the Swiss Alps is more extensive than that of any other living geologist.—(*Address at the Anniversary Meeting of the Royal Geographical Society, 24th May* 1852, *by Sir R. I. Murchison*, p. 45.)

14. *Salt Lake of Utah.*—While engaged upon this duty, we frequently enjoyed the luxury of bathing in the water of the lake. No one, without witnessing it, can form any idea of the buoyant properties of this singular water. A man may float, stretched at full length, upon his back, having his head and neck, both his legs to the knee, and both arms to the elbow, entirely out of the water. If a sitting position be assumed, with the arms extended to preserve the equilibrum, the shoulders will remain above the surface. The water is nevertheless extremely difficult to swim in, on account of the constant tendency of the lower extremities to rise above it. The brine, too, is so strong that the least particle of it getting into the eyes produces the most acute pain; and if accidentally swallowed, rapid strangulation must ensue. I doubt whether the most expert swimmer could long preserve himself from drowning, if exposed to the action of a rough sea.

Upon one occasion a man of our party fell overboard into the lake, and, although a good swimmer, the sudden immersion caused him to take in some mouthfuls of water before rising to the surface. The effect was a violent paroxysm of strangling and vomiting, and the man was unfit for duty for a day or two afterwards. He would have inevitably been drowned, had he not received immediate assistance. After bathing, it is necessary to wash the skin with fresh water, to prevent the deposit of salt arising from evaporation of the brine. Yet a bath in this water is delightfully refreshing and invigorating.

The analysis of this water by Dr Gale, has shewn that it contains rather more than 20 per cent. of pure chloride of sodium, and not more than 2 per cent. of other salts, forming "one of the purest and most concentrated brines known in the world." Its specific gravity was 1·17, but this will slightly vary with the seasons, being doubtless affected by immense floods of fresh water which come rushing down into it from the mountains, in the spring, caused by the melting of the snows in the gorges.—(*Stansburg's Expedition to the Valley of the Great Salt Lake of Utah*, p. 212.)

15. *Suggestion that all Africa has a grand Basin-like Arrangement.*—Sir R. I. Murchison, in his Address at the Anniversary Meeting of the Royal Geographical Society, on the 24th May 1852, under the head, *Comparative View of Africa in Primeval and*

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Modern Times, gives an interesting statement of the circumstances which have led him to the important general suggestion of the *Basin-like arrangement of all Africa.*

ZOOLOGY.

16. *Agassiz appointed Professor of Comparative Anatomy in the Medical College of the State of South Carolina.*—At a special meeting of the Trustees and Faculty of the Medical College of the State of South Carolina, held on the 3d day of January 1852, Dr L. Agassiz was unanimously elected Professor of Comparative Anatomy, with the distinct understanding that the collegiate expenses of the student are not to be increased by this addition to the course. These lectures are therefore free to the medical students, the College paying, from their funds, the Professor.

A sketch of Professor Agassiz's intended course is here subjoined.

The course will consist of a sketch of the natural classification of the Animal Kingdom, with full illustration of the fundamental differences of their four great types, the Radiates, Molluscs, Articulates and Vertebrates. Confident in the doctrine that the essential functions of life are performed by systems of organs, which differ fundamentally in the different types of animals, the professor will describe their structure separately, in the successive classes, and not follow the ordinary course of connecting in one series the various apparatus performing similar functions. Beginning with the Radiata, he will shew how the *plan* of their structure, as well as the structure itself, differs entirely from that of the other three great types, as these also differ among themselves. Taking the Polyps as the lowest class, he will illustrate the theory of the cellular structure of all animals, by a comparison of the microscopic structure of the various tissues of higher animals in the progress of formation, with that of the perfect and permanent condition of the lower ones. The class of Medusæ will afford him an opportunity of illustrating the phenomena of alternate generation, and also of testing the foundation of the natural relationship between animals, upon which their division into classes is based; whilst the study of Echinoderms, and their position at the head of Radiata, without the possibility of a transition to either Molluscs or Articulates, will afford ample evidence that there is no *one* gradual series among animals, from the lowest to the highest.

The type of Molluscs will lead to general considerations, respecting the bilateral symmetry of animals, and the different tendencies manifested in the type of Articulates, when contrasted with Molluscs. The characteristic peculiarities of structure of these two important divisions of the animal kingdom will be fully illustrated, and their respective position as natural groups investigated. The Molluscs will lead particularly to an inquiry into the communications between internal cavities of these animals and the surrounding media, and between the different systems of organs themselves. The Articulates again

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will lead naturally to the consideration of metamorphoses in general, and the successive changes through which particular types of animals pass during the different periods of their life. The importance of tracing these changes throughout the animal kingdom, in order fully to appreciate the relative standing of the different families in each class, will also be made prominent. A full account of intestinal worms will complete the history of Articulates.

Particular attention will finally be given to the structure of Vertebrates, their affinities and homologies, and the natural progressive gradation which may be traced between their four classes, from Fishes through Reptiles, Birds and Mammalia, to Man. Besides considering the structure of these animals in their adult condition, full information will be given upon their embryonic growth, from the first formation of the egg to the final development of the germ; thus affording another opportunity of tracing the remarkable parallelism which exists between the different stages of growth of animals belonging to the same great type, and the different degrees of development which their different families present in their full-grown condition.

Constant reference will be made to the structure of the human body, in order to prepare the student more fully for a correct understanding of its peculiarities and the functions of its organs. The lectures will be illustrated by numerous diagrams, and the exhibition of natural specimens.

This Course of Lectures was delivered to a very crowded and enthusiastic audience.

MISCELLANEOUS.

17. *Galvani and Volta.*—No one who wishes to judge impartially of the scientific history of these times and of its leaders, will consider Galvani and Volta as equals, or deny the vast superiority of the latter over all his opponents or fellow-workers, more especially over those of the Bologna school. We shall scarcely again find in one man gifts so rich and so calculated for research as were combined in Volta. He possessed that "incomprehensible talent," as Dove has called it, for separating the essential from the immaterial in complicated phenomena; that boldness of invention which must precede experiment, controlled by the most strict and cautious mode of manipulation; that unremitting attention which allows no circumstance to pass unnoticed; lastly, with so much acuteness, so much simplicity, so much grandeur of conception, combined with such depth of thought, he had a hand which was the hand of a workman.

18. *Sir Charles Lyell's visit to North America.*—Sir Charles Lyell has just left England for North America. The objects of the journey are the examination of the geology of some extensive tracts of country in the United States, and in Canada, and of delivering courses of lectures on geology in that country.

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Books and Maps published and to be published.

19. *Dr Thomson's Narrative of a Journey through the Mountains of Northern India during the years* 1847–8.—This valuable and deeply interesting work, of which more on a future occasion, we very earnestly recommend to the attention of Naturalists and Geographers. Many of the books of travels supplied by the press are harmless evanescences, forming a striking contrast to the enduring pages of Dr Thomson.

20. *Professor Charles Upham Shepard's Treatise on Mineralogy.*—The mineralogies of Cleaveland, Alger, and Dana, are well known, and highly esteemed in this country, and so also is that of Professor Shepard. We have now before us Part 1st of the 3d edition of that valuable work, which we strongly recommend to the students of Mineralogy, with the remark, that the course pursued by Professor Shepard in his Mineralogy, is worthy of adoption by other mineralogists at no great distance from us.

21. *Humboldt's Cosmos.*—All will rejoice to learn that the illustrious Humboldt has recovered from his late indisposition, and that this extraordinary man, although about 83 years of age, is now actively employed in preparing the fourth and last volume of his renowned work, *Cosmos.*

22. *Silurian System.*—Sir R. I. Murchison is preparing a new work on his Silurian System.

23. *Bischof's Chemical Geology.*—An English edition of this celebrated work will soon appear under the patronage of the Camden Society.

24. *Professor Bischof's Work on Natural Science.*—A third edition of this excellent popular view of Natural Science has just appeared in Germany. A translation of it would, we are convinced, be favourably received by a numerous class of readers in this country.

25. *Map of the Distribution of Plants and Animals.*—Professor Edward Forbes exhibited and explained to the meeting of the British Association in Belfast, a very interesting map illustrative of the distribution of Organic Beings throughout the land and waters of our planet. This map, we understand, is to be engraved and published by Mr Keith Johnston of Edinburgh.

26. *Smithsonian Contributions to Science.*—The third and fourth volumes of this valuable work, presented by the Smithsonian Institution to the Wernerian Society, have just been received. The contents of these volumes are as follow :—

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VOL. III., 4to, 1852.

1. Observations on Terrestrial Magnetism. By John Locke, M.D.
2. Researches on Electrical Rheometry. By A. Secchi.
3. Contributions to the Natural History of the Fresh Water Fishes of North America. By Charles Girard.
4. Nereis Boreali Americana, or Contributions to a History of the Marine Algæ of North America. By William Henry Harvey, M.D.
5. Plantæ Wrightianæ Texano Neo-Mexicanæ. By Dr Asa Gray, M.D. Part I.
6. The Law of Deposit of the Flood Tide: Its Dynamical Action and Office. By Charles Henry Davis, Lieut. U. S. Navy.
7. Description of Ancient Works in Ohio. By Charles Whittlesey.
8. Occultations visible in the United States during the year 1852. Computed by John Downes, Esq.
9. Ephemeris of Neptune for the year 1852. By Sears C. Walker, Esq.

VOL. IV., 4to, 1852.

A Grammar and Dictionary of the Dakota Language. Collected by the Members of the Dakota Mission. Edited by Rev. S. R. Riggs, A.M., Missionary of the Am. Board in Foreign Missions.

The Smithsonian Institution announce that shortly a fifth quarto volume of the Smithsonian Contributions to Knowledge will appear. The papers in this volume are,—

1. Plantæ Fremontianæ; or Descriptions of Plants collected in California by Col. J. C. Fremont. By John Torrey, F.L.S.
2. On Entophyta in Living Animals. By Dr Joseph Leidy. Ten Plates.
3. On the Winds of the Northern Hemisphere. By Prof. J. H. Coffin. With 27 Plates.
4. On the Fossil Vertebrata of the Fresh-Water Eocene of Nebraska. By Dr Joseph Leidy. Ten Plates.
5. On the Nervous Anatomy of Ranapepiens. By Dr Jeffreys Wyman. Two Plates.
6. On the Fossil Cetaceans of the United States. By Prof. L. Agassiz.
7. On the Structure of the Coral Animal. By Prof. L. Agassiz. Six Plates.

27. *Espy's Second Report on Meteorology, &c.*—This valuable work, in 65 pp. fol., with numerous coloured charts, has just reached this country. Also the Smithsonian Report on Recent Improvements in the Chemical Arts, by Professor James C. Booth and Campbell Morfit, have been received by the Wernerian Society.

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List of Patents granted for Scotland from 22d June to 22d September 1852.

1. To JOHN DAVIE MORRIES STIRLING, of Black Grange, N. B., Esq., "certain alloys and combinations of alloys."—22d June 1852.

2. To ALFRED VINCENT NEWTON, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "improvements in separating substances of different specific gravities," being a communication.—23d June 1852.

3. To JOHN HENRY JOHNSON, of 47 Lincoln's Inn Fields, in the county of Middlesex, and of Glasgow, N. B., gentleman, "improvements in steam-engines," being a communication.—28th June 1852.

4. To JOHN LINTORN ARABIN SIMMONS, of No. 67 Oxford Terrace, Hyde Park, in the county of Middlesex, captain in the Royal Engineers, and THOMAS WALKER, of the Brunswick Iron Works, Wednesbury, in the county of Stafford, Esq., "improvements in the manufacture of ordnance, and in the construction and manufacture of carriages, and traversing apparatus for manœuvreing the same."—28th June 1852.

5. To FREDERICK SANG, of 58 Pall Mall, in the county of Middlesex, artist in fresco, "improvements in machinery, or apparatus for cutting, sawing, grinding, and polishing."—30th June 1852.

6. To PETER BRUFF, of Ipswich, in the county of Suffolk, civil engineer, "improvements in the construction of the permanent way of rail, tram, or other roads, and in the rolling stock or apparatus used therefor."—5th July 1852.

7. To GEORGE LAYCOCK, late of Albany, in the United States of America, dyer, but now of Doncaster, in the county of York, tanner, "improvements in unhairing and tanning skins."—6th July 1852.

8. To ROBERT JOHN SMITH, of Islington, in the county of Middlesex, "certain improvements in machinery or apparatus for steering ships and other vessels."—7th July 1852.

9. To JAMES HIGGIN, of Manchester, in the county of Lancaster, manufacturing chemist, "certain improvements in bleaching and scouring woven and textile fabrics and yarns."—8th July 1852.

10. To WM. BECKETT JOHNSON, of Manchester, in the county of Lancaster, managers for Messrs Ormerod and Son, engineers and iron-founders, "improvements in railways and in apparatus for generating steam."—12th July 1852.

11. To RICHARD PARIS, of Long Acre, in the county of Middlesex, modeller, "improvements in machinery or apparatus for cutting and shaping cork."—12th July 1852.

12. To PETER ARMAND LE COMTE DE FONTAINE MOREAU, of No. 4 South Street, Finsbury, London, in the county of Middlesex, and 39 Rue de l'Ex-

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chequier, Paris, patent agent, "improvements in the apparatus for kneading and baking bread and other articles of food of a similar nature," being a communication.—13th July 1852.

13. To Alfred Vincent Newton, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "improvements in machinery for cutting soap into slabs, bars, or cakes," being a communication.—15th July 1852.

14. To Richard Laming, of Mill Wall, in the county of Middlesex, chemist, "improvements in the manufacture and the burning of gas in the treatment of residual products of such manufacture, and of the distillation of coal or similar substances, and of the coking of coal, and in the application of a certain substance which may be obtained from such treatment to the manufacture of paper."—19th July 1852.

15. To Emery Rider, of Bradford, in the county of Wilts, manufacturer, "improvements in the manufacture or treatment of india-rubber and gutta-percha, and in the applications thereof," being a communication.—19th July 1852.

16. To Charles Augustus Preller, of Abchurch Lane, in the city of London, gentleman, "improvements in the preparation and preservation of skins and animal and vegetable substances."—19th July 1852.

17. To Wm. Reid, of University Street, electric engineer, and Thomas Watkins Benjaman Brett, of Hanover Square, gentleman, "improvements in electric telegraphs."—19th July 1852.

18. To Peter Armand le Comte de Fontaine Moreau, of No. 4 South Street, Finsbury, London, in the county of Middlesex, and 39 Rue de l'Exchequier, Paris, patent agent, "certain improvements in railways and locomotive engines, which said improvements are also applicable to every kind of transmissions of motion."—21st July 1852.

19. To Richard Archibald Brooman, of the firm of J. C. Robertson and Co., of 166 Fleet Street, in the city of London, patent agent, "improvements in the purification and decoloration of oils, and in the apparatus employed therein," being a communication.—21st July 1852.

20. To William Septimus Losh, of Wreay Syke, in the county of Cumberland, gentleman, "improvements in obtaining salts of soda."—21st July 1852.

21. To Joseph Maudsley, of the firm of Maudsley, Sons, and Field, of Lambeth, in the county of Surrey, engineers, "improvements in steam-engines, which are also applicable wholly or in part to pumps and other motive machines."—21st July 1852.

22. To Robert Hesketh, of Wimpole Street, St Marylebone, in the county of Middlesex, "improvements in apparatus for reflecting light into rooms and other parts of buildings and places."—22d July 1852.

23. To Edward Maitland Stapley, of Cheapside, "improvements in cutting mouldings, tongues, and other forms, and in planing wood," a communication.—22d July 1852.

24. To Joseph Haythornr Reed, late 17th Lancers, Harrow Road, in

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the county of Middlesex, gentleman, "improvements in propelling vessels."—2d August 1852.

25. To WILLIAM EDWARD NEWTON, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, "improvements in the construction of wheels for carriages," being a communication.—3d August 1852.

26. To JOHN GERALD POTTER, of Over Darwen, in the county of Lancaster, carpet manufacturer, and MATTHEW SMITH, of the same place, manager, "certain improvements in the manufacture of carpets, rugs, and other similar fabrics."—August 5, 1852.

27. To RALPH ERRINGTON RIDLEY, of Hexham, in the county of Northumberland, tanner, "improvements in cutting and reaping machines."—5th August 1852.

28. To WILLIAM ACROYD, of Birkenshaw, near Leeds, "improvements in the manufacture of yarn and fabrics, when cotton, wool, and silk are employed."—6th August 1852.

29. To A. V. NEWTON, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, mechanical draughtsman, "improvements in the manufacture of metallic fences, which improvements are also applicable to the manufacture of verandahs, to truss frames for bridges, and to other analogous manufactures," being a communication.—13th August 1852.

30. To ROBERT HARDMAN, of Bolton-le-Moors, in the county of Lancaster, mechanic, "improvements in looms for weaving."—18th August 1852.

31. To JAMES PILLING, of Rochdale, in the county of Lancaster, spinner and manufacturer, "certain improvements in looms for weaving."—20th August 1852.

32. To JOSEPH WILLIAM SCHLISINGER, of Buxton, in the county of Surrey, gentleman, "improvements in fire-arms, in cartridges, and in he manufacture of powder."—26th August 1852.

33. To FREDERICK SANG, of No. 58 Pall Mall, in the county of Middlesex, artist in fresco, "certain improvements in floating and moving vessels, vehicles, and other bodies on and over water."—26th August 1852.

34. To JOSEPH DENTON, of Prestwick, in the county of Lancaster, gentleman, "certain improvements in machinery, or apparatus for manufacturing looped terry, or other similar fabrics."—26th August 1852.

35. To ALEXANDER PARKES, of Birmingham, "improvements in separating silver from other metals."—26th August 1852.

36. To JAMES WARREN, of Montague Terrace, Mile End Road, gentleman, "improvements applicable to railways and railway carriages, and improvements in paving."—26th August 1852.

37. To THOMAS RICHARDSON, of Newcastle-on-Tyne, "improvements

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in the manufacture and preparation of magnesia and some of its salts." —26th August 1852.

38. To Alexander Stewart, of Glasgow, in the county of Lanark, North Britain, manufacturer, "improvements in the manufacture or production of ornamental fabrics."—27th August 1852.

39. To Sir John Scott Lillie, Companion of the Honourable Order of the Bath, of Pall Mall, "certain improvements in the construction or covering of walls, floors, roads, foot-paths, and other surfaces."—31st August 1852.

40. To Pierre Isidore David, of Paris, in the republic of France, machinist, "certain improvements in the method of bleaching, and in the apparatus connected therewith."—1st September 1852.

41. To Joshua Crockford, of Southampton Place, in the county of Middlesex, gentleman, "improvements in brewing and brewing apparatus."— 2d September 1852.

42. To Thomas Wilkes Lord, of Leeds, in the county of York, flax and tow machine maker, "improvements in machinery for spinning, preparing, and heckling, of flax, tow, hemp, cotton, and other fibrous substances, and for the lubrication of the same, and other machinery."— 6th September 1852.

43. To Edmund Morewood and George Rogers, of Enfield, gentlemen, "improvements in the manufacture, shaping, and coating of metals, in applying sheet metal to building purposes, and in the means of applying heat."—6th September 1852.

44. George Wright, of Sheffield, and also of Rotherham, in the county of York, artist, "improvements in stoves, grates, or fire-places." —11th September 1852.

45. To Thomas Hunt, of Leman Street, Goodman's Fields, in the county of Middlesex, gentleman, "improvement in fire-arms."—13th September 1852.

46. To Alexander Mills Dix, of Salford, in the county of Lancaster, brewer, "certain improvements in artificial illumination, and in the apparatus connected therewith, which improvements are also applicable to heating and other similar purposes."—16th September 1852.

47. To John M'Conochie, of Liverpool, in the county of Lancaster, engineer, "improvements in locomotive and other steam-engines and boilers in railways, railway carriages, and their appurtenances, also in machinery and apparatus for producing part or parts of such improvements."—20th September 1852.

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